A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

VOL. II.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN-COURT, TEMPLE-BAR; FOR

THE AUTHOR, No. 10, SOHO-SQUARE;

AND SOLD BY

G. AND J. ROBINSONS, PATERNOSTER-ROW.

1802.

PREFACE.

I MEET my Readers and Correspondents at the completion of this Second Volume, with the satisfaction of directing their notice to the still greater increase of communication, and the means of rendering this work most eminently interesting and useful. It is not necessary that I should recapitulate the outlines of the plan and rules of conduct by which I have endeavoured to deserve the encouragement of the Public; but I think it truly justifiable to exhibit the marks of that approval at the same time that I express the sensibility with which I accept them, and the spirit they must afford to my labours.

Without entering into any estimate of the quantity of original matter in this work, which from the high value of its correſpondence is now become the authentic repository of the researches of our philosophers; I will only notice, that the number of original writers in this Volume is more than double that of the reſpectable list prefixed to the first Volume, at the same time that the value and importance of the Memoirs from foreign and domestic publications have continued to increaſe.

The authors of original Papers are Mr. F. Accum; A. B. C; John Bostock, M. D. J. W. Boswell; Count de Bournon; H. Campbell, M. D.; R. Chenevix, Esq.; Mr. John Clennel; C. P.; Mr. Wm. Close; W. Cruickshank, Prof. at Woolwich; John Cuthbertson; D. H.; J. Fletcher, Eſq. G. H; John Gough, Esq.; Mr. Olinthus Gregory; J. C. Hornblower; Rob. Jameson; J.; Rev. W. Pearson; N. N. Joseph Priestley, L.L.D. F.R.S. &c.; H. Sarjeant, Esq.; Mr. Tho. Sheldrake; Dr. J. H. Schroeter; Tho. Thomson, M.D.; Mr. Trevithick; Troughton; Ez. Walker; Rev. James Wilson, D.D. James Woodhouse, M.D.; Mr. Arthur Woolf; Thomas Young, M. D. Prof. P. R. I.; Baron von Zach; and W. N.—Of foreign works, Carcel; Carreau; Coulomb; Descroizilles; Guillot; Guyton; Hassel Lachenaie; Lalande; Pictet; Proust; Valentine; Vauquelin.—And of English Memoirs abridged or extracted, Mr. Banks; Wm. Bullock; Sir H. Englefield, Bart.; Mr. Gilpin; J. Gough, Esq.; Ch. Hatchett, Esq.; Wm. Herschel, L.L.D. Ed. Howard, Esq.; Hulme, M.D.; E. Jones, Esq. H. Sarjeant, Esq.

The novelty and excellence of the communications with which this Journal has been honoured, have been productive

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tive of an effect which I have seriously meditated to remedy In the limited extent of every work of this nature, when the new and interesting Memoirs demand a larger portion of its capacity, a smaller must of course be devoted to less striking, though doubtless very important business of selection. Foreign and domestic matter must be more fastidiously sorted out; articles must be abridged instead of being given at full length; and some must be rejected altogether that would have been highly acceptable, if the original productions could have allowed room.

Two remedies present themselves. The first is to print a supplementary number to each volume; and the other to give a greater number of pages without adding to the price. I shall be happy to adopt the latter as soon as the increased ſale shall have rendered it practicable, without diminishing the ordinary remuneration the Work affords; and in case the former should prove necessary or advisable, I am confident my Readers will see the advantages and approve the proceeding. In the mean time, the private recommendation of men of merit to their friends, is the best means of accelerating that circulation which will eventually benefit its patrons, by the greater quantity of them that could in that case be afforded.

I conclude this Preface as usual, by mentioning the ſubjects of the sixteen Plates which illustrate the present Volume. 1. Improvements in Hydraulic Engines, by Mr. Boswell. 2. Guyton's Improvements of the Swediſh Stove. 3. Mr. Sarjeant's cheap Engine for raising Water. 4. Strong framed Levers for Steam Engines, by Mr. Hornblower. 5. Dr. Young's Diagrams to illustrate the Theory of Light. 6. Mr. Gregory's Figure for Mr. Pearſon's Analogy. 7. Mr. Gough's Illustration of the Doctrine of Sound. 8. Mechanical Lamp of Carcel and Carreau. 9. Mr. Woolf's Apparatus for heating Water by waste Steam. 10. Mr. Terry's improved Mill. 11. Mr. Bullock's Lock. 12. Count Bournon's Figures of Anhydrous Sulphate. 13. Mr. Trevithick's Application of a temporary Forcer. 14. Lachenaie's Apparatus for claying Sugars. 15. Mr. Jameson's Illustration of the Formation of Granite. 17. Mr. Banks's Instruments for determining the Pressures and Velocities of effluent Air or Gas. 18. The Spirit Hydrometer and Scales of Atkins. And, 19. Compound Condensers of Electricity, by Mr. Read and Mr. Cuthbertson.

Soho Square, London, September 1, 1802.

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A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

MAY, 1802.

ARTICLE I.

Improvements in the Hydraulic Engine of SCHEMNITZ, *and that of Mr.* GOODWYN; *with comparative Remarks on the most useful Applications of each, and some Facts relative to the Invention of the pressure Engine. In a Letter from Mr.* JOHN WHITLEY BOSWELL.

To Mr. NICHOLSON.

SIR,

London, March 14. 1802.

HAVING, with much satisfaction, found that the method of making the Schemnitz hydraulic engine work itself, which I gave you for your excellent Journal in 1800, (IV. 117.) has been since found of considerable utility in * other works of a similar nature, I am induced to send for your approbation a draft of the † application of the same principle to Mr. Good- Method of working engines by the bucket and syphon.

* Vide Mr. Close's papers in the same Vol.

† The paper signed L in the quarto Journal, page 343, though it professes to shew how Mr. Goodwyn's engine may work itself, has only hinted at this method, but has not shewn how it may be effected.

wyn's engine, and another of a method of caufing the Schemnitz to raife water *above* the level of the prime refervoir, together with a comparative view of the advantages of both engines and their powers.

Concife explanation of the Schemnitz engine.

After a perufal of my former Paper on the Schemnitz engine, (IV. 144.) a mere infpection of the figure given here (Plate I.) will be fufficient to fhew the manner in which this now propofed will operate. The moving power is the preffure of the column of water from the refervoir R, (Fig. 1.) to D in the lower air chamber A, which forces the air contained in it into the chamber B, which air fo compreffed in B will impel the water contained in it upwards through the pipe to a height, and in a quantity proportionate to the relative height of the column of water contained in the pipe R D, compared with that contained in S B, or fuppofing the length of R D given, the greater the length of the pipe S B is, (fo as not to exceed R D,) the lefs will be the quantity of water delivered at S, and *vice verfa.*

Mr. Goodwyn's engine; conftructed for heavy work, and made to operate without attendance.

In the draft of Mr. Goodwyn's engine, Fig. 2. I have endeavoured to exhibit it as it fhould be if executed on a large fcale, and made all the pipes detached from each other, becaufe though the plan of making one pipe pafs through another and through the refervoirs, made ufe of in Mr. Goodwyn's model, is very convenient and neat in an apparatus that may be placed on a table, yet it would be found to produce an unneceffary trouble, complication, and difficulty of repair in a large engine. The method fhewn in this draft of caufing the engine to work without attendance, is the fame as for the Schemnitz, and caufes the cocks G and H to open at intervals, (which may be regulated at pleafure by the hand cock I, letting the water flow more or lefs quick into the fyphon veffel E,) while at the fame time it clofes the cock at D, and *vice verfa.* Self-moving valves are placed at the delivering pipes of the chambers C and B, and alfo at the air vent of A, becaufe wherever they can be ufed they are preferable to cocks, or valves ufed by external power ; fome doubt may arife, whether there fhould not be a paffage for the air let at intervals into A, as well as one for it to efcape : but as great quantities of air are contained in water, which the mode of working of this engine will particularly tend to feparate from it, I think it would be needlef and that the felf-moving valve

valve opening outwards at the air vent of A will be ſufficient. The pipe at K is to conduct water from the bucket F to that of the cock D. There are two ranges of reſervoirs repreſented, to ſhew the method of raiſing water by this engine about thirty feet high: more would be uſeleſs, and even a ſecond would in very few inſtances be found neceſſary, except when the fall of water from R to D was very ſhort, in which caſe it would be better to uſe ſome other engine for raiſing water to the required height.

Compariſon of the powers of theſe two engines; numerically ſtated.

In comparing thoſe two engines, it will be found that their powers and capabilities are nearly ſimilar.

1. In both the greater the height of the original fall of water, denoted by the pipe R D, and the greater the quantity of water which it can ſupply in a given time, the greater quantity can be raiſed by theſe engines in a given time.

2. Both engines can be conſtructed ſo as to raiſe water above the original level, and from below to the ſurface, or from a pit.

3. By a ſucceſſive number of reſervoirs both engines can be brought to raiſe water to any height; but as they will raiſe a ſmaller quantity as the height is increaſed, the quantity wanted in a given time, and the expence of conſtruction, will limit the extent of their elevation.

4. In both engines the diſtance from one reſervoir to another, muſt always be leſs than that of the original fall R D. The circumſtances in which thoſe engines differ ariſe from the difference of their manner of action.

5. The Schemnitz engine operates by cauſing a fall of water to *compreſs* air, which re-acting on other water forces it to riſe in a pipe to a certain height. Mr. Goodwyn's engine acts by cauſing a fall of water to *rarify* a certain quantity of air, into whoſe ſpace the preſſure of the atmoſphere forces, when permitted, a quantity of water.

6. Hence in the Schemnitz engine, the preſſure acting from *within outwards,* tends to *burſt* the veſſels uſed in the ſtructure, and to *open* and *extend* any fiſſures which may chance to be in them.

7. In Mr. Goodwyn's engine the preſſure acting from *without inwards,* cloſes all the parts of which it is compoſed more together, tends to make its pipes and veſſels more ſtaunch, and in any fiſſure makes its ſides operate like valves to ſhut it up.

 8. The

8. The Schemnitz engine will alwa's raiſe water to a height nearly equal to that of the original all, from one reſervoir to another, ſuppoſing the original fallof any height whatſoever, as 100 feet.

Mr. Goodwyn's engine will not raiſe water from one reſervoir to another ſo high as thirty feet in ary caſe whatſoever, as there cannot be a complete vacuum forned by it in the air chamber, but only an approximation to one

Mr. Goodwyn's engine will be preferable, and leaſt coſtly in ſmall elevations.

From this compariſon it will follow, tha wherever the original fall of water is leſs than thirty-two fee, Mr. Goodwyn's engine will be much preferable to the Schemitz; as, from the 7th article of the compariſon, it may be mde of the *cheapeſt materials*, of ſtrong *wooden caſks* and *wooten pipes*; whereas the Schemnitz engine, from the 6th article, muſt be made of the ſtrongeſt, and of courſe moſt coſtly matrials, of caſt iron at leaſt, and that of conſiderable thickneſs.

The Schemnitz engine for greater depths.

But wherever the original fall exceeds the height of thirty feet much, and it is required to raiſe the waer to nearly the ſame height, then the Schemnitz engine appears to be preferable; as, in all probability, the fewer number of parts which it will, in this caſe, require in its conſtrction, will more than compenſate for its coſtly materials.

Piſton engine beſt for very great depths.

When it is required to raiſe water to a height, much greater than that of the original fall, above the firſt evel, or from a greater depth; either from the original fall beng ſhort, or the required height being great, an engine in which the preſſure of the water is made to act by a piſton in an apparatus ſimilar to that of the ſteam engine, (one of which is deſcribed in your Journal for March) will be preferable to either of the above.

Compariſon of the three engines.

The compariſon of thoſe engines may be brought to this one point: wherever Mr. Goodwyn's engine can be uſed, with a ſingle continued pipe for elevating the water, and without a ſucceſſion of reſervoirs, it ſeems to be the cheapeſt.

Where Mr. Goodwyn's engine cannot be uſed without violating this condition, but the Schemnitz can, it promiſes to be the next in point of cheapneſs, from its ſimplicity, abſence of friction, and ſmall number of working parts.

But when neither Mr. Goodwyn's engine nor the Schemnitz can be uſed without a number of reſervoirs, then the piſton preſſure engine probably ought to be preferred; but this will much depend on the number of reſervoirs, for perhaps one or

two

two in addition to the Schemnitz might coſt leſs, than boring the cylinder of the piſton engine perfect, and its additional machinery: for merely raiſing water the powers of each are nearly equal, depending all on the height of the original fall of water.

The great advantage of the piſton preſſure engine is, not as a cheap engine for raiſing water, but as that in which a fall of water can be applied without any *waſte* to work mills or machinery for *any purpoſe;* which is of very great conſequence when the fall of water is of conſiderable height, and the ſtream, or ſupply, ſmall.

The preſſure engine works *without waſte of water.*

Conceiving it of very great importance to have it determined in what ſituations each of the principal engines, worked by water preſſure, is to be preferred, I have commenced this compariſon, and if this ſhall be acceptable, will ſend another paper on a ſimilar ſubject; that is, on the compariſon of the common mill water wheel, with another mode of applying the water to turn mill work, which, I think, I can demonſtrate to be much preferable. I hope what I have thus began will excite ſome others to the ſame enquiry; and that by this means the multiplicity of water preſſure engines will be at laſt arranged, and their comparative utility aſcertained, ſo that in every different caſe of fall or ſupply of water, an engineer may know at once which he ſhould uſe.

Great importance of comparing the different methods of applying water as a force.—Wheel work, &c.

I beg leave to add here ſome remarks on the piſton preſſure engine in your Journal for March: Mr. Trevithack will, I hope, excuſe my taking from him the honour of his being the firſt inventor of this mode of applying a fall of water, to give it back to Meſſrs. Deniſard and Deuille of France, when I confeſs that for a long time I was in the ſame error with him, and thought it had firſt occurred to me, and propoſed it with that idea to Mr. Carnac in Nov. 1796, to draw the water from his copper mine, (by which he would have ſaved the daily labour of twenty men, as he had a fall of water very proper for this engine—a fact which I have Mr. Carnac's ſignature to prove;) but I ſince found out that in Belidor's Hydraulic Architecture, publiſhed at Paris 1739, in the fourth book and firſt chapter, there is a method deſcribed at large, with very well executed plates, by which a fall of water operating in a cylinder on a piſton may work a pump to force water to a greater height; and what is remarkable, Belidor

Mr. Trevithack's preſſure engine was executed in 1731 by Deniſard and Deuille—and imagined by Mr. Boſwell in 1796.

Deſcribed in Belidor.

 propoſes

proposes the very same method of working the valves by a tumbling weight, as that called the tumbling bob, in Mr. T's description. In Belidor's engine the piston cylinder and pump are both horizontal, which is the most material difference, but the principle is entirely the same as Mr. Trevithack's.

Mr. Belidor does not claim the honour of this invention, but only proposes the engine I allude to, as an improvement on one executed on the same principle at Seve, between Versailles and Paris, in the year 1731, by Denisard and Deuille, for which they obtained a patent from the king of France for twenty-one years; of which engine there is also a description in the same chapter of the above work.

Proposal of air chambers in the piston.

I beg leave to suggest, that it might be a considerable improvement to this engine to have its action made elastic, by the addition of an air chamber, on the same principle as that used in engines for extinguishing conflagrations; such a one, seems to me, might be best effected by making the piston hollow, and of a larger size, to serve for this purpose, as the air spring would then act both on the upper and lower pressure of the water; Figure 3 is a sketch of this method, in which A represents the hollow piston.

I hope the length of this paper will be excused by the circumstance of my not having received your Journal for March till I had written the most of it, and having had of course, to add the remarks on the piston pressure engine to the rest.

I am, SIR, &c.

W. H. B.

II.

Remarks of the present State of Paper-making in England and France. By* H. CAMPBELL, *M. D. Communicated by the Author, March* 26, 1802.

Whether the additional duty on paper has principally injured book trade.

THE additional duty on Paper has not been the chief cause of the diminution in the book and paper trades of this country. After

* The importance and authenticity of the chemical, mechanical, and other facts stated in this Memoir, and of the object itself in general

After eſtabliſhing this fact, it will be equally evident that the paper and book trades will not be *effectually* relieved by giving up a part, or the whole of the laſt additional duty.

The former adminiſtration of this country by impoſing an additional duty, did not teach the French the new art of paper making; nor did it ſupply them with abundance of raw material; or ſhew them the art of equalizing the different qualities of rags by Chaptal's mode of bleaching;—neither was any new light thrown by the additional duty upon the mode of printing and type caſting, as now practiſed in France.

France has ſucceeded by improvements in the arts of paper making and printing.

Before the commencement of the late war, paper-making in this country, ſtrictly ſpeaking, was a mechanical art. The ſuperiority of Engliſh paper aroſe from the ſuperior linen worn by Engliſh people. Their rags were ſuperior to the rags collected on the continent. The decency of the Engliſh populace, compared with the populace on the continent, could not be better ſhewn than by an exhibition of Engliſh and foreign rags. Rags called *London fines*, the refuſe of Iriſh linen, &c. could ſcarce be equalled in any other country. The original linen of theſe rags had been highly bleached. They were conſequently calculated to make the fineſt and whiteſt paper. Rags at an Engliſh paper mill previous to the war, were *ſorted* according to their colour and fineneſs. Pains were taken to cut off the ſeams and offal parts; and theſe parts were deſtined to make inferior paper. This ſtatement alone is almoſt ſufficient to give a clear notion of what I am about to eſtabliſh.

The Britiſh fine paper formerly excelled from the fineneſs of our *linen rags*; and our careful ſorting them.

I conceive that a repreſentation of the *mechanical neceſſaries*, as the duſter, the knives placed in the engine roller, the plate, the vat, the moulds, and laſtly, flannels and preſſes, although indiſpenſible in a paper mill, are neceſſary to be noticed in this account; as an imitation of them in France, could not contribute to the preſent alarming change in our paper trade.

Our machinery,

The Engliſh ſtaple, ſuperior rags, Engliſh cleanlineſs in cutting and aſſorting, and better engines and knives, conſti-

attempted to be transferred to France.

general to the progreſs of ſcience and literature, leave no doubt reſpecting the propriety of inſerting it in our Journal, though its moſt prominent feature relates to political regulation. On this laſt ſubject my conduct as a Journaliſt cannot be ſuppoſed to expreſs any opinion as an individual.---N.

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tuted our ſuperiority. About the year 1789 and 90, *certain reſpectable* Engliſh paper makers endeavoured to take theſe advantages to France. France apparently required them; her coarſe foul cotton rags were not made better by ſeparation and aſſortment; her engines were deficient, and her mills exhibited no mark of proſperity, but every ſymptom of ſlovenly neglect.

French rags are *coarſe cotton.*

French paper for copper plates.

One ſort of paper, notwithſtanding the want of colour and cleanlineſs, ſhe excelled in, upon neceſſity. I mean paper for copper plate prints; ſhe neceſſarily excelled becauſe her ſtaple cotton rag being more bibulous, received better impreſſions from the plate. This was the ſtate of her paper trade and mills in the year 1789. Before that period, ſhe did not conſume all her own rags. We received a conſiderable part from the cellars of Dunkirk and Oſtend, and from countries in her ſouthern vicinity, Leghorn, &c.

New æra in paper making. The bleaching proceſs by ox. mur. acid.

About this time a new æra in paper making commenced. Chemiſtry by her diſciples, Scheele, Berthollet, and Chaptal, from a metallic oxide, ſolicited and directed the concentrated and pure part of the atmoſphere, oxigen, to remove with expedition the colouring part of cloth, or rags made from vegetable ſubſtances, ſuch as flax, cotton, hemp, &c. An attention to the beſt bleaching proceſs ought certainly to form a material part of the conſiderations in this Paper; becauſe this object, connected with a knowledge of the ſorts of rags ſuitable for bleaching, and making *printing* paper, would give that comprehenſion of the evil from which might be drawn —not ſteps of temporary and unavailing expedience, but ſolid foundations of relief.

Coarſe cotton rags are brought to an equality with *fine linen* rags by that proceſs.

The buſineſs under conſideration is more intimately connected with printing than with writing paper. The bleaching gas is much better adapted to coarſe cotton rags than coarſe linen or hempen rags; becauſe the former is without ligneous particles, and the latter abounds with them, and theſe particles, called by paper makers *ſheaves,* are made more conſpicuous by bleaching. The ſtaple of our opponents the French, conſiſts in coarſe cotton rags. I can, if required, point out places where depots ſhould be eſtabliſhed for affording an unlimited ſupply of ſimilar *cheap* materials, and where each ſhip, by way of finiſhing her lading, will take on board a convenient number of bags.

A plentiful and cheap ſupply of cotton rags.

In

In the year 1793-4, I imported a parcel of cotton rags at 9l. per ton; bleaching them added 8 per cent. to the 9l. The bleached ſtuff was worth more than 40l. per ton: the beautiful paper produced now exiſts in a public work. If paper makers, or ſtationer-paper makers, had paid attention to this new and growing improvement; (or would pay attention) inſtead of ſoliciting a remiſſion of duties, they might be able with their *capital* and mills ready formed, to counteract the French, and contribute ſtill more to the ſtate, than is paid at preſent. A relaxation of part of the duties, cannot be a radical relief. To forego an object of finance without remedying a complaint, can only be a compliance with the government, to participate in misfortune with the petitioners.

Inſtance in point. The true remedy, ſtated to conſiſt in the adoption of the improvements, and the proper employ of our capital and works.

The neceſſity of aboliſhing improper combinations and power among journeymen paper makers, and the injury done to paper mills by London ſtationers *importing and regulating the price of rags,* and monopolizing mills, are evident directions to relieve the ſuffering part of the trade, and how to obtain revenue from the wealthy part, by way of granting licences, I ſhall be happy, if required, to communicate.

Combinations and monopolies. Hints at *regulations by the State.*

The arts, commerce, and navigation of this country, are juſtly conſidered by the French government, to be the ſinews of England. Chaptal, Berthollet, and other enlightened men are encouraged to affect us, and benefit their own country in theſe particulars: that they have not been unſucceſsful is manifeſted by the ſtate of the paper trade in France.

General obſervations. The arts, commerce, navigation.

The maxim of trade finding its level, is too ſupine a maxim in the preſent ſtate of diſcoveries. Factories are altering, and markets muſt vary. Goods formed by mere *mixture,* ſuch as ſaline and other bodies, require neither capital, nor machinery. Many of theſe are in a ſtate of great cultivation in France. In arts reſting on capital, machinery, and aptitude of hand, we ſhall long remain unrivalled.

Level of trade. What manufactories can be eaſily transferred; and what are the moſt permanent in a wealthy country.

At preſent I ſhall check theſe obſervations, that my remarks on the paper trade may remain diſtinct and unmixed with any other matter.

H. CAMPBELL.

No. 11, Fleet-Street, London.

III. *Remarks*

III.

Remarks on Combuſtion, by THOMAS THOMSON, *M. D. Lecturer on Chemiſtry in Edinburgh.*

Phenomenon of combuſtion is very ſtriking to every claſs of men.

NO operation of nature has a better claim to our attention than COMBUSTION. The irreſiſtible devaſtation which it ſometimes occaſions is calculated to ſtrike the ignorant with terror; the extraordinary changes which it produces naturally attract the inquiſitive eye of the philoſopher, while its ſubſerviency to almoſt every branch of domeſtic economy renders it a familiar and neceſſary agent in the hands of every individual. This familiar acquaintance with combuſtion ſeems, however, to have retarded the inveſtigation of its nature; for it was not till the ſeventeenth century that philoſophers made it a ſerious object of enquiry. The labours of Bacon, Boyle, Hooke, and Mayow are well known; and the ſucceſs with which theſe labours were attended, muſt, if we recollect the difficulties to be overcome, give us a very high idea of the genius of theſe inveſtigators of nature. But the philoſophers of our own age, eſpecially Lavoiſier, have gone far beyond their predeceſſors; and have explained ſome of the moſt intricate and important phenomena of combuſtion.

Eminent inveſtigators of its theory.

Lavoiſier's theory

Mr. Lavoiſier's theory of *combuſtion*, improperly termed his theory of *chemiſtry*, is ſo generally known, that it is unneceſſary to enter into any detail concerning it. Its merit is indiſputable, and raiſes its author to the very firſt rank among philoſophers. Many chemiſts ſeem to think that it explains the whole phenomena of combuſtion; but an attentive examination muſt convince every impartial obſerver, that the theory of Lavoiſier, ingenious and ſatisfactory without doubt, as far as it goes, leaves yet ſeveral parts of that very complicated proceſs as unaccountable as ever. He has corrected the errors of his predeceſſors, and made a very important new ſtep; but many new ſteps are ſtill wanting to render the theory complete. I hope, therefore, that the following remarks will not be conſidered as altogether improper; they will at leaſt exhibit the ſubject in a new point of view, and may perhaps contribute to call the attention of chemiſts to certain phenomena

leaves much to be ſtill elucidated.

phenomena which have not hitherto been claffified, nor examined with that precifion to which they are entitled.

The French confine the term combuftion to the act of oxigenation.

1. Though the French chemifts have lately given the term *combuftion* a new meaning, and made it ftand for the general *combination of a body with oxigen*, I mean, for reafons which will appear hereafter, to employ it in the fenfe ufually affixed to the term by the generality of mankind. Now when a body undergoes combuftion, in the common fenfe of the word, two things always take place. 1. The body gradually waftes away, and often difappears altogether; it is then faid to be *confumed* or *burnt*. 2. During the whole of this procefs it emits *heat* and *light*; the heat and light thus emitted are ufually denominated *fire*, and the wafte of the body is confidered as the effect or confequence of its combuftion. If either of thefe two phenomena be wanting, we do not fay in common language that a body is undergoing combuftion, or that it is burning. Every theory of combuftion then muft explain, 1. Why the burning body is wafted and altered. 2. Why during the progrefs of this alteration heat and light are emitted.

Ufual acceptation of the term preferred, *confumption or wafte with heat and light.*

The theory muft explain thefe effects.

2. If we take a view of the different bodies which occupy the attention of chemifts, we fhall find, that as far as combuftion is concerned, they may be arranged under three claffes; namely, 1. Combuftibles. 2. Supporters of combuftion. 3. Incombuftibles.

Relative to combuftion bodies are 1. combuftibles, or 2. fupporters of combuftion, or 3. incombuftible.

I. The COMBUSTIBLES are thofe bodies, which are faid in common language to *burn*. During combuftion they appear to emit light and heat, and at the fame time gradually wafte away. When this change has reached its maximum, the procefs of combuftion is at an end. The clafs of combuftibles is very numerous; but all the bodies belonging to it may be fubdivided into three fets; namely,

I. *Combuftibles* or the bodies confumed.

1. Simple combuftibles,
2. Compound combuftibles,
3. Combuftible oxides.

The *fimple combuftibles* are twenty-four or twenty-five in number, namely,

They are fimple combuftibles; or

1. Sulphur, 3. Carbon,
2. Phofphorus, 4. Hidrogen gas,
5. All the metals *.

* Except perhaps gold, filver, and mercury.

The

—compound combuftibles,

The *compound combuftibles* confift of compounds formed by the fimple combuftibles uniting together two and two; and are of courfe much more numerous than the fimple combuftibles. They may be arranged under the five following heads:

1. Sulphurets, 3. Carburets,
2. Phofphurets, 4. Alloys,
5. Sulphurated, phofphorated, and carbonated hidrogen.

or combuftible oxides,

The *combuftible oxides* are compofed of one or more fimple combuftibles, combined with a dofe of oxigen. Though the French chemifts have given to thefe bodies the name of *oxides*, we fhall fee afterwards that they differ effentially from *metallic oxides* and from water, which is confidered at prefent as an oxide of hidrogen. The combuftible oxides may be arranged under two heads: 1. Thofe which contain only a fingle bafe combined with oxigen, and which therefore may be termed *fimple combuftible oxides*. 2. Thofe which contain more than one bafe combined with oxigen, and which therefore may be termed *compound combuftible oxides*.

and thefe laft are either fimple, having a fingle bafe, or compound, having more than one bafe.

Simple comb. oxides.

The fimple combuftible oxides are only four in number; namely,

1. Oxide of fulphur, 3. Charcoal,
2. Oxide of phofphorus, 4. Carbonic oxide gas.

Unlefs fulphur, phofphorus, and hidrogen gas, bodies at prefent confidered as fimple, belong to this clafs. All the fimple combuftible oxides are by combuftion converted into acids.

Compound comb. oxides.

The compound combuftible oxides include by far the greater number of combuftible bodies; for almoft all the animal and vegetable fubftances belonging to them. The double bafe is ufually carbon and hidrogen: alcohol, ether, refins, gums, &c. are inftances of compound combuftible oxides *.

Phlogifton.

It was believed by Stahl and his difciples, that all combuftible bodies contain one common principle, to which they owe their combuftibility. But in confequence of the difcoveries of Lavoifier this theory has been laid afide.

II. *Supporters* of combuftion

II. The *fupporters of combuftion* are a fet of bodies which are not of themfelves, ftrictly fpeaking, capable of undergoing combuftion, but which are abfolutely neceffary for the procefs; for no combuftible body can be made to burn unlefs

* To this clafs of bodies alfo muft be referred all the vegetable and animal acids.

fome

ſome one or other of the *ſupporters* be preſent. Whenever they are excluded the proceſs ſtops. All the ſupporters known at preſent are the following ſix :

1. Oxigen gas, 4. Nitrous gas *,
2. Air, 5. Nitric acid,
3. Gaſeous oxide of azote, 6. Oximuriatic acid.

There are indeed certain ſubſtances beſides theſe, which poſſeſs nearly the ſame properties; theſe I ſhall enumerate afterwards under the title of *partial ſupporters*.

All the ſupporters contain one common principle, namely, *oxigen*. The firſt of them conſiſts of oxigen uncombined with any baſe ; but in the other five the oxigen is united to a baſe. It is very remarkable, that in four caſes out of five, the baſe to which the oxigen in theſe compound ſupporters is united is *azote*. Is it not probable from analogy, that oximuriatic acid, the remaining compound ſupporter, contains azote likewiſe as a component part. universally contain oxigen. N. B.

III. The *incombuſtible bodies* are neither capable of undergoing combuſtion themſelves, nor of ſupporting the combuſtion of thoſe bodies that are; of courſe they are not immediately connected with combuſtion. At preſent we are acquainted with about 13 incombuſtible bodies, not reckoning the compounds which they are capable of forming with each other. Theſe are, III. *Incombuſtibles.*

1. Azotic gas, 3. The earths.
2. The fixed alkalies,

The firſt of theſe ſubſtances conſtitutes the baſe of almoſt all the compound ſupporters. Some of the alkalies and earths poſſeſs certain properties in common with combuſtibles, and are capable of exhibiting phenomena ſomewhat analogous to combuſtion ; phenomena to be deſcribed afterwards under the title of *ſemi-combuſtion*. Semi-combuſtion.

3. From the preceding obſervations it is obvious, that in every caſe of combuſtion there muſt be preſent a *combuſtible* and a *ſupporter*. Now during combuſtion the combuſtible always unites with the oxigen of the ſupporter. It is this combination which occaſions the apparent waſte and alteration of the combuſtible. The new compound thus formed I ſhall call a *product of combuſtion*. Now every product of combuſtion is either, Combuſtion requires a *combuſtible* and a *ſupporter*. Product of combuſtion is either *water* or an *acid*, or a *metallic oxide*.

* Mr. Davy firſt proved that this gas is a ſupporter.

either, 1. *water*, or 2. *an acid*, or 3. *a metallic oxide.* It is true indeed, that other bodies ſometimes make their appearance during combuſtion, but theſe will be found upon examination not to be products, nor to have undergone combuſtion.

Thus one of the two characteriſtic marks which diſtinguiſh combuſtion, namely, the apparent waſte and alteration of the combuſtible body, has been fully explained. For the explanation of it we are indebted to Lavoiſier. It conſtitutes what is uſually, but abſurdly, termed the *new theory of chemiſtry*, and is the moſt important ſtep which has been made towards a complete theory of combuſtion,

Facility of combuſtion is not proportioned to the attraction for oxigen.

But though the combination of the combuſtible with oxigen be a conſtant part of combuſtion, yet the facility with which combuſtibles burn is not proportional to their apparent affinity for oxigen. Phoſphorus, for inſtance, burns more readily than charcoal; yet charcoal is capable of abſtracting oxigen from phoſphorus, and of courſe has a greater affinity for it. The combuſtible oxides take fire more readily than ſome of the ſimple combuſtibles; thus charcoal burns more eaſily than carbon or diamond: alcohol, ether, and oils, are exceedingly combuſtible, whereas all the metals require a very high temperature when the ſupporter is air. This greater combuſtibility of combuſtible oxides is probably owing to the weaker affinity by which their particles are united. For the cohesion of heterogeneous particles, when oxigen conſtitutes a part of them, is uſually weaker than the coheſion of homogeneous particles. Hence they are more eaſily ſeparated than homogeneous particles, and of courſe combine more readily with oxigen; thoſe ſimple combuſtibles which melt eaſily, or which are in the ſtate of elaſtic fluids, are alſo very combuſtible, becauſe the coheſion between their particles is eaſily overcome.

But chiefly depends on the facility of deſtroying the coheſion.

Hence *compound ſupporters* are more readily burned.

It is owing to the ſame inferiority in the coheſion of heterogeneous particles, that ſome of the compound ſupporters occaſion combuſtion in circumſtances when the combuſtibles would not be acted on by ſimple ſupporters. Thus phoſphorus burns in air at the common temperature; but it does not burn in oxigen gas, unleſs the temperature exceed 90°. In oximuriatic acid gas phoſphorus burns rapidly at the common temperature of the air, and ſo do ſeveral of the metals;

though they cannot be made to burn in air except at a very high temperature. Thus alſo oils burn rapidly when mixed with nitrous acid. Nitrous gas and the gaſeous oxide of azote conſtitute exceptions to this rule.

4. None of the *products* of combuſtion are combuſtible according to the definition of combuſtion which I have given. This want of combuſtibility is not owing to their being ſaturated with oxigen; for ſeveral of them are capable of combining with an additional doſe of it. But during this combination no caloric nor light is ever emitted; and the compound formed differs eſſentially from a *product* of combuſtion; for by this additional doſe of oxigen the *product* is converted into a *ſupporter*.

Products of combuſtion are never combuſtible.

Oxigenation of a *product* converts it into a *ſupporter*.

Hence we ſee that combuſtion ought not to be confounded with the combination of a body with oxigen, as is done by the French chemiſts. Combuſtion indeed cannot take place without the combination of oxigen; but oxigen may combine without combuſtion. Thus when iron is burnt, it always combines with 0.27 of oxigen, and is converted into the *black oxide*, a product of combuſtion, and altogether incombuſtible; capable, however, of combining with an additional doſe of oxigen, and of being converted into the *red oxide*. But during this laſt combination, how rapidly ſoever it takes place, no heat nor light is emitted. Now the *red oxide of iron* is not a product of combuſtion, but a ſupporter; as the following experiments demonſtrate: Mix it with phoſphorous, and put the mixture into the bottom of a long glaſs tube, ſhut at one end, and filled with azotic gas. Cloſe the mouth of the tube, and apply heat to that part in which the mixture is. At a certain temperature a violent detonation takes place, which ſhatters the tube in pieces. It is needleſs to remark, that the tube muſt be ſufficiently long to prevent the effects of expanſion in the gas included.

Difference between combuſtion and oxigenation.

Detonation of phoſphorus with oxide of iron.

When antimony is burnt, it always combines with 0.20 of oxigen, and is converted into the *white oxide*. Now this white oxide, which is a product of combuſtion, and of courſe incombuſtible, is capable of combining with an additional doſe of oxigen, and of being converted into the *acidulous oxide* of antimony. In like manner, lead, when burnt, is converted into the *white oxide* of lead, a product; but this product combines with

Other *products* converted into *ſupporters*.

with additional doses of oxigen, and is converted into the *red* and *brown* oxides, both of which are supporters. When the *supporters*, thus formed by the combination of oxigen with *products*, are made to support combustion, they do not lose all their oxigen, but only the additional dose which constituted them supporters. Of course they are again reduced to their original state of products of combustion. Hence it follows, that they owe their properties as supporters, not to the whole of the oxigen which they contain, but to the additional dose which constituted them supporters. We may therefore call them *partial supporters*, indicating by the term, that part only of their oxigen is capable of supporting combustion, and not the whole. It is very possible that both *azote* and *muriatic acid* may be products of combustion; and in that case both the compound and partial supporters would agree with each other in every respect. In the present state of our knowledge, however, it is necessary to distinguish them.

They act by part of their oxigen; and are *partial supporters.*

The bases of all known *partial supporters* are metallic.

All the partial supporters with which we are acquainted, contain a metallic basis; for metallic oxides are the only products at present known capable of combining with an additional dose of oxigen. It is a circumstance highly deserving of attention, that when metals are capable of combining with several doses of oxigen, the product or oxide formed by combustion is seldom or never that which contains a maximum of oxigen. The following oxides are products of combustion:

Enumeration.

1. Black oxide of iron.
2. White oxide of zinc.
3. White oxide of lead.
4. Yellow oxide of tin.
5. White oxide of antimony.
6. White oxide of arsenic.
7. White oxide of manganese.
8. Oxide of copper *.
9. Oxide of cobalt *.
10. Oxide of nickel *.
11. Oxide of bismuth *.
12. Purple oxide of gold ?
13. Yellow oxide of silver ?
14. Black oxide of mercury †?

* The particular oxide of these metals, which is the product of combustion, has not been ascertained; but they are all combustible in oximuriatic acid gas.

† I doubt much whether gold, silver, and mercury, be combustible at all. They do not burn in air, how high soever the temperature is; neither do they detonate with red hot nitre, nor exhibit any appearance of combustion in oximuriatic acid gas; though this last body oxidates them with great rapidity.

The

The following oxides, on the other hand, are partial supporters of combustion:

1. Red oxide of iron *.	6. Red and brown oxides of lead **.
2. Yellow oxide of gold †.	7. Black oxide of manganese.
3. White oxide of silver ‡.	8. Acidulous oxide of antimony?
4. Red oxide of mercury §.	9. White oxide of tin?
5. Arsenic acid ‖.	

This list would doubtless be increased by an accurate examination of all the metallic oxides not included in either of these tables.

Incombustibility of *products* is not owing to want of affinity for oxigen.

Thus it appears that several of the products of combustion are capable of combining with oxigen. The incombustibility of products, therefore, is not owing to their want of affinity for oxigen, but to some other cause.

Nor can they *support* combustion, though they can combine with combustibles.

5. No *product* of combustion is capable of *supporting* combustion. This is not occasioned by any want of affinity for combustible bodies; for several of them are capable of combining with an additional dose of their basis. But by this combination they lose their properties as products, and are converted into *combustibles*. The process therefore differs essentially from combustion.

Instances. This combination affords *partial combustibles*, acting by part of their basis.

Thus sulphuric acid, a *product* of combustion, by combining with an additional dose of sulphur or its oxide, is converted into *sulphureous acid*, a substance which, from several of its properties, I conclude to be combustible. Thus also phosphoric acid, a product of combustion, is capable of combining with phosphorated hidrogen, and of forming *phosphorous* acid a combustible body. When this last acid is heated in contact with a supporter, it undergoes combustion; but it is only the additional dose of the combustible which burns, and the whole is converted into phosphoric acid. Hence we see that it is not the whole basis of these compounds which is combustible, but merely the additional dose. The compounds, therefore, formed by the union of a product and combustible,

* Fulminates with phosphorus.
† Forms fulminating gold.
‡ Forms fulminating silver.
§ Forms fulminating mercury.
‖ Occasions combustion when heated with several combustibles.
** Occasions the combustion of sulphur.

may

may be termed *partial combuſtibles*; indicating by the name, that a part only of the baſe is capable of undergoing combuſtion. Now ſince the products of combuſtion are capable of combining with oxigen, but never exhibit the phenomena of combuſtion except when they are in the ſtate of partial combuſtibles, combuſtible bodies muſt contain ſome principle which they loſe during combuſtion, and to which they owe their combuſtibility; for after they have loſt it, they unite to oxigen without exhibiting the phenomena of combuſtion.

Since *products* can combine with oxigen, but never exhibit combuſtion unleſs they be *partial combuſtibles*, combuſtibles muſt contain a ſubſtance which they loſe in burning.

Products can give oxigen to *combuſtibles*, and convert them into products; but they do not, like *ſupporters*, cauſe combuſtion.

Though the products of combuſtion are not capable of ſupporting combuſtion, they not unfrequently part with their oxigen juſt as ſupporters do, give it out to combuſtibles, and convert them into products; but during this proceſs no heat nor light is ever evolved. Water, for inſtance, gives out its oxigen to iron, and converts it into *black oxide*, a product; and ſulphuric acid gives out its oxigen to phoſphorus, and converts it into phoſphoric acid. Thus we ſee that the oxigen of products is capable of converting combuſtibles into products, juſt as the oxigen of ſupporters; but during the combination of the laſt only, are heat and light emitted. The oxigen of ſupporters then contains ſomething which the oxigen of products wants.

The oxigen of *ſupporters* differs from that of *products*.

Combuſtibility is reſtored to *products* only by *combuſtibles*.

6. Whenever the whole of the oxigen is abſtracted from products, the combuſtibility of their baſe is reſtored as completely as before combuſtion; but no ſubſtance is capable of abſtracting the whole of the oxigen, except a *combuſtible* or a *partial combuſtible*. Water, for inſtance, is a product of combuſtion, whoſe baſe is hidrogen. To reſtore the combuſtibility of the hidrogen, we have only to mix water with iron or zinc filings; the metal is oxidated, and the hidrogen gas is evolved as combuſtible as ever. But no ſubſtance, except a combuſtible, is capable of ſeparating hidrogen gas from water by combining with its oxigen. In the ſame manner phoſphorus abſorbs the oxigen from ſulphuric acid, and is converted into a product, while ſulphur is ſeparated in its uſual ſtate of combuſtibility. Thus we ſee that combuſtibles are capable of reſtoring the combuſtibility of the baſes of products, but they themſelves loſe their combuſtibility by the proceſs, and are converted into products. Combuſtibility, therefore, may be thrown at pleaſure from one body to another. This fact was firſt ſet in a proper light hy Stahl, and was the great ſtep in the theory of combuſtion

Doctrine of Stahl.

combuſtion for which we are indebted to that philoſopher. Some miſtakes into which he fell were afterwards corrected by Lavoiſier *.

When *products* are thus formed without combuſtion, a new combuſtible is evolved.

From theſe facts it is obvious, that the products of combuſtion may be formed without combuſtion; but in theſe caſes a new combuſtible is always evolved. The proceſs is merely an interchange of combuſtibility; for the combuſtible is converted into a product only by means of a product. Both the oxigen and the baſe of the product having undergone combuſtion, have loſt ſomething which is eſſential to combuſtion. The proceſs is merely a double decompoſition. The product yields its oxigen to the combuſtible, while at the ſame time the combuſtible gives out ſomething to the baſe of the product; the combuſtibility of that baſe then is reſtored by the loſs of its oxigen, and by the reſtoration of ſomething which it receives from the other combuſtible thus converted into a product.

Complicated caſe of products formed by nitric acid, &c.

There is indeed another method of forming the products of combuſtion without actual combuſtion in certain caſes; but the phenomena are much more complicated. This method is to expoſe them to the action of ſome of the ſupporters diſſolved in water; eſpecially nitric acid. Thus moſt of the metallic oxides may be formed without combuſtion by the action of that acid on the metals. But in that caſe a new ſupporter is always evolved, namely, nitrous gas; ammonia, a new combuſtible, is uſually alſo formed; and not unfrequently the *product* is converted into a *partial ſupporter*.

Oxigen may combine without loſing the ingredient which occaſions combuſtion.

7. No *ſupporter* can be produced by combuſtion, or by any equivalent proceſs. Now as all the ſupporters, except oxigen gas, conſiſt of oxigen combined with a baſe, it follows as a conſequence, that oxigen may combine with a baſe without loſing that ingredient, whatever it is, which gives occaſion to combuſtion. The act of combination of oxigen with a baſe, therefore, is by no means the ſame with combuſtion. If we

* When ſulphate of iron is dropt into a ſolution of muriate of gold, nitrate of ſilver, or nitrate of mercury, the gold, ſilver, and mercury are precipitated in the metallic ſtate. This is an additional reaſon for ſuſpecting that theſe three metals are not combuſtible. Every perſon, however, muſt have obſerved, that the metals in queſtion have not the metallic luſtre when firſt precipitated, and that they acquire it ſlowly when allowed to remain expoſed to the light.

All supporters procured either from other supporters, or by electricity.

take a view of the different supporters, we shall find that all of them which can be obtained artificially, are procured either from other supporters, or by the agency of electricity.

Oxigen.

I. OXIGEN GAS may be procured from nitric acid and oximuriatic acid, two supporters; and from several of the partial supporters, as the black oxide of manganese, the red oxides of lead and of mercury. The action of heat is always necessary; but the process is very different from combustion.

Air.

II. AIR, as far as is known at present, cannot be formed artificially. The gas indeed which comes over during part of the usual distillation of nitre and sulphuric acid to obtain nitrous acid, resembles air very closely. But it is obtained from a supporter.

Oxide of azote.

III. The GASEOUS OXIDE of azote, or nitrous oxide of Davy, has hitherto been only procured from nitrous gas and nitric acid (nitrate of ammonia), both of which are supporters.

Nitrous gas.

IV. NITROUS GAS can only be procured by the decomposition of nitric acid, a supporter.

Oximuriatic acid.

V. OXIMURIATIC ACID can be formed by combining muriatic acid with the oxigen of nitric acid, a supporter; or with the oxigen of the black oxide of manganese, the red oxides of lead, iron, mercury, &c. all of which are partial supporters.

Nitric acid formed spontaneously.

VI. NITRIC ACID is formed spontaneously upon the surface of the earth by processes with which we are unacquainted.

(To be continued.)

IV.

Some Account of a new Planetary Body, discovered by DR. OLBERS, *on the 28th of March,* 1802.

Discovery of a new planet.

A LETTER from Dr. J. H. Schroeter, of Lilienthal, to Mr. Best, London, was lately read at the Royal Society, in which he gives an account of this remarkable discovery of a second planetary body of small apparent magnitude, discovered by Dr. Olbers, of Bremen, on the 28th of last March, when it formed the south point of an equilateral triangle with the stars Nos. 20 and 19 of the constellation Virgo. During the comparisons he was induced to make, from his conviction that it was not there in January when he re-discovered the Ceres

near

near the ſame ſpot, he found its change of place to be very perceptible. The following obſervations were tranſmitted by him to Dr. Schroeter:

	d.	h.	m.	s.						
1802. Mar.	28	9	25	10	M. T.	App. R. Aſ.	184°	56′	49″	Its place.
						App. Dec.	11	33	30 N.	
	29	8	49	14	M. T.	App. R. Aſ.	184	46	36	
						App. Dec.	11	52	59 N.	

Dr. Olbers had not the means of obſerving any diſc, but Dr. Schroeter, from his information, began to obſerve it on the 30th with his thirteen feet reflector, while his aſſiſtant, Mr. Harding, determined its place. With a power of 288 of this large inſtrument, it appeared round and leſs hazy than Ceres, with a diameter of 4.635 ſeconds, which is much larger than thoſe of Ceres and the Georgium Sidus; the former of which, on the 28th March, meaſured 4.021 ſec. and the latter, on the 20th, meaſured 3.973 ſeconds. Its light, though pale and white in compariſon with that of Ceres, was nevertheleſs more intenſe upon the whole, as appeared by its projection on the diſc micrometer; but the Georgium Sidus was much brighter.

Its apparent diameter larger than that of Ceres or the Geo. Sidus.

Its light.

A minute ſtar was ſeen near it on two ſeveral days, which Dr. Schroeter ſeems to ſuſpect as a ſatellite.

Suſpicion of a ſatellite.

The poſition of the Olberian planet (or comet), at March 30 d. 8 h. 20 m. 50 ſec. mean time, was

App. R. A. 184° 35′ 52″ and 12° 15′ 8″ app. N. declin.

On the 1ſt of April it was again obſerved with the great reflector, and appeared to exhibit no diſc, but was leſs in a brighter light with the power of 288, and could not be diſtinguiſhed from a fixed ſtar. When it ſometimes appeared with a diſc, its diameter was only 3″.244. The Doctor is diſpoſed to aſcribe this to the heavy dew.

No diſc on April 1.

Letter from Mr. W. WALKER, *Lecturer on the Eidouranion.*

To Mr. NICHOLSON.

41, *Conduit Street, Hanover Square, London,*

SIR, *April* 26, 1802.

THE account you did me the favour of inſerting in your valuable Journal of the laſt month, reſpecting the ſituation of the planet Ceres, will be rendered additionally intereſting at preſent, from the circumſtance of a ſtill more recently obſerved

moving

moving ſtar being ſituated very near to the ſituation there pointed out as the place of the Ceres on the 25th of March.

Preſent ſituation, April 26.

The planet or comet diſcovered by Dr. Olbers, at Bremen, on the 28th ult. is at preſent in a very ſmall degree to the S. E. of the ſituation of the Ceres on the 25th of March, and will readily be ſeen by a night-glaſs or teleſcope. It is inviſible to my naked eye, but appears of a definable diſc with a magnifying power of a 100 times. Its light is pale red and very faint, and through the night-glaſs is leſs brilliant than the Ceres, although no magnifying power that I can uſe will give the latter any apparent diameter. It ſeems probable that its diſtance is about as far again as the Earth is from the Sun, whilſt the Ceres is near three times the diſtance, and Mars about once and a half as far off. As my object is merely to enable any perſon to find it, I do not trouble you with any more full account at this late period of the month.

Definable diſc with power of 100. Pale red and leſs brilliant than Ceres. Suppoſed to be nearer the Earth.

I remain, SIR,
With much reſpect,
Your conſtant reader,
W. WALKER.

Preſent ſituation of Ceres.

The Ceres Ferdinandia will be found a little to the north-eaſt of the ſtar Beta in the Lion's Tail, being the eaſtermoſt point of a right angled triangle formed by Beta, a double ſtar due north of it and itſelf.

V.

On Bradley's Method of obſerving Tranſits, and another Method by which the Thickneſs of the Wire is rendered of no Importance, In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Dr. Bradley's method of making tranſit obſervations deſcribed.

THE method of taking tranſit obſervations introduced by Dr. Bradley is ſtill uſed by aſtronomers. This conſiſts in noting the proportional diſtance of the ſtar from the wire at the two beats of the clock, one immediately preceding, and the other immediately following its paſſage acroſs the wire. If the wire be ſo thick as to cover the ſtar, and the ſtar happens to be behind it when the clock beats, the ſituation of the ſtar's

ſtar's centre cannot be exactly known, which makes the introduction of fine wires very deſirable. The fineſt wires ſtill proving too thick for very ſmall ſtars, an aſtronomical friend of mine hinted to Mr. Troughton that he might probably receive aſſiſtance in this delicate branch of his buſineſs from ſome of his ſpiders. This hint was not loſt,*—and hence Bradley's method of obſerving ſeems to be carried to the higheſt degree of perfection by the aſſiſtance of a harmleſs inſect which is perſecuted without mercy by every *fille de chambre* throughout his Majeſty's dominions.

Spider's webs in the foci of teleſcopes.

There is, however, another method of obſerving which precludes the neceſſity of very fine wires. This conſiſts in noting the time when the centre of the ſtar comes to the SIDE of the wire. But before this method is uſed it is neceſſary that one ſide of the middle wire ſhould be brought into the meridian; ſuppoſe it be that ſide which appears to the weſt when the teleſcope is turned to the ſouth, then the obſervations are to be taken on that ſide of all the wires.

Another method of obſerving one ſide of the wire.

It is a *line drawn by the ſtrength of imagination* down the middle of the wire, parallel to the ſides, which is uſed in Bradley's method, but in my method a *real line* is preſented to the eye of the obſerver and which he ſees very diſtinctly, although as fine as if it had been drawn through a geometrical definition.

Which is a real line.

This method of obſerving ſeems to be more ſimple than the other, in conſequence of its being, in many caſes only neceſſary to attend to one ſide of the wire: for example: Should the clock beat when half the ſtar is covered by the wire, it is evident that the centre of the ſtar paſſes the ſide of the wire at that time. And by making uſe of the apparent diameter of the ſtar, and the thickneſs of the wire as two meaſures, the fractional part of a ſecond may be eſtimated, by an experienced obſerver to a very great degree of preciſion.

Its advantages.

How far this ſmall alteration in the method of uſing the tranſit teleſcope may be found convenient to others experience muſt determine; but for my own part, I am certain that I can obſerve, not only with more eaſe to myſelf, but with greater exactneſs by this method than by Bradley's.

I am, Sir,

Your very humble ſervant,

Lynn, April 19, 1802. EZEKIEL WALKER.

* See a further account of this invention in the laſt vol. of this Journal, pa. 319.

VI. *Deſcription*

VI.

Defcription of a Stove on the Principles of the Swedifh Fire-place, with Heat-openings, by CITIZEN GUYTON.*

Fire-places in France not generally conftructed on good principles.

THE true principles of conftructing fire-places, fo as to obtain the greateft heat with the leaft confumption of fuel, have been known for fome time in France; but they have been much lefs generally adopted, than the neceffity for economifing fuel demands. We fee many fire-places fo deep as to confume double the quantity of fuel neceffary, and yet heat the apartment but faintly, where half the expence might be fpared by altering the fire-place according to count Rumford's plan.

Often too deep.

Smoky chimneys how ufually remedied.

If a chimney fmoke, inftead of reducing the tunnel to proper dimenfions, fo that defcending currents cannot take place in it, fcarcely any remedy is thought of but air-holes, which require the facrifice of a certain quantity of fuel, to counterbalance the effect of the cold air continually entering.

The ufe of the Swedifh ftoves is probably yet rare, from their not having been conftructed on juft principles, or in the beft proportions, at their firft introduction. As I have had one made, which appears to many of my friends to produce an aftonifhing effect, in compliance with their requeft I fhall give an exact defcription of it, premifing however a few principles with regard to fires.

General principles with regard to fires for domeftic purpofes.

1. The heat produced is proportionate only to the air confumed by the fuel.

2. The quantity of heat produced by a given quantity of fuel is greateft when the combuftion is moft complete.

3. The combuftion is moft complete when the filiginous part of the fuel is retained longeft in pipes in which it may undergo a fecond combuftion.

4. Of the heat produced none is of ufe, but what is diffufed through the fpace to be heated, and retained in this fpace.

5. The temperature in this fpace will be higher, in proportion as the current of air, which is to renew and keep up the combuftion, is lefs difpofed to abforb the heat of this fpace in paffing through it.

* Abridged from the Annales de Chimie, vol. xli. p. 79. C.

Hence

Hence we deduce the following obvious conſequences: Corollaries deduced.

1. The fire-place muſt be kept ſeparate from all bodies that conduct heat rapidly.

2. As heat can be produced only by combuſtion, and combuſtion can be maintained only by a current of air, this current ſhould be attracted into pipes, where it preſerves the requiſite velocity, without going away from the place to be heated; ſo that the heat it depoſits in it gradually accumulates in the whole of the iſolated ſtove, to be afterward given out ſlowly, according to the laws of its equilibrium

3. When the wood is conſumed to ſuch a point as to afford no more ſmoke, it is of advantage to ſtop the outlets of theſe pipes, to keep in the heat, which would be carried into the chimney by the continued current of freſh air, which would neceſſarily be of a lower temperature.

4. We ſhall obtain a higher temperature, and preſerve it longer, under ſimilar circumſtances, if we conſtruct within the ſtove, or under the hearth and round the fire-place, pipes in which the air derived from without is warmed before it enters into the apartment to ſupport the fire, or to replace what has been conſumed. Apertures for heated air.

Theſe pipes are what have been called heat openings, *(bouches de chaleur,)* becauſe inſtead of conſidering their principal object, it is commonly ſuppoſed, that they are made to give a more rapid paſſage to the heat produced. This is not totally without foundation, ſince the temperature of the air iſſuing from them is increaſed by the heat it abſorbs from the ſtove; and on this account ſome might be diſpoſed to neglect them, as contrary to the moſt eſſential object, that of retaining the heat in it; but it is to be obſerved, that we can ſhut theſe outlets when we pleaſe; and that we may even cut off all communication with the external air by means of a ſimple ſlider; ſo that every advantage may be derived from them without any inconvenience. It muſt be added, that they are neceſſary in very cloſe apartments, unleſs we would expoſe ourſelves to currents of cold air. Theſe reaſons have induced me to employ the heat openings in the Swediſh ſtove, to which they had not been applied. Neceſſary in very cloſe apartments.

The Swediſh ſtoves are conſtructed ſtrictly according to the trueſt principles, and the pipes in which the ſmoke circulates are diſpoſed in the beſt manner for effecting its complete combuſtion. Swediſh ſtoves conſtructed on the beſt principles.

So ufeful, that they are become general in Sweden;

buftion. Their utility has been found fo great, that they have become general in Sweden, where the winters are very fevere, and where they have diminifhed the confumption of wood one third, fo that there is no country where the inclemency of the weather is guarded againft at lefs expence.

and are employed in different works.

They have likewife been employed advantageoufly, with the neceffary variations of form, in dye-houfes, breweries, &c.

Conftruction;

Their conftruction is by no means expenfive; they fave iron-work, and require only bricks or tiles. Thefe are recommended to be placed edgewife, and chofen as thin as poffible for the inner walls. The circulating pipes are to be placed fo, that rain falling down the chimney can never get into them.

and method of ufing them.

The method of ufing them is fo eafy, that in the largeft public buildings one perfon is fufficient to light all the fires. All the wood that can be contained in the fire-place, which is very fmall, is to be put in at once; it is to be fawn into pieces of equal lengths; and as foon as it is burned, the flider that ftops the communication of the circulating pipes with the chimney is to be thruft in. By thefe means all the heat, which the fuel is capable of producing, remains in the pipes, and iffues out flowly, and only to diffufe itfelf in the apartment; while a fingle piece of wood, that had not burned at the fame time with the reft, would oblige the flide to be left open, and the current of air neceffary for its combuftion would carry off into the chimney the greater part of the heat produced.

Defcription of the Swedifh ftove conftructed by Guyton.

The following is a defcription of the ftove conftructed under my directions.

Its height,

Fig. 1, Plate III. reprefents a front view of the ftove: its height is 164 centimetres (about 61 inches French), exclufive of the vafe, which is a feparate ornament, merely placed on the top.

breadth,

Its breadth is 85 centimetres (about $31\frac{1}{2}$ inches.)

depth.

Its depth 58 centimetres (about $21\frac{1}{2}$ inches.)

Height may vary.

The height may vary according to the fize of the apartment, and be extended without inconvenience to two metres (about 6 feet, 2 inches.) It may likewife be reduced, as I have done for ftoves in a laboratory, which were to fupport a fand bath as high as the hand.

Proportions of the circulatory pipes.

The other two dimenfions are determined by thofe of the bricks employed to form the interior circulatory pipes, which fhould be in certain proportions, that the fmoke may pafs through

through them freely, without ſo much air entering with it as would condenſe it, or ſink the temperature below the degree neceſſary for combuſtion.

Heat openings.

V V are the external parts of the two heat openings.

m m Apertures of the ſtove, by which the air, that is to iſſue through the heat openings, enters. Theſe are cloſed when the air is drawn from without through a pipe paſſing under the floor; which is much more advantageous for renewing the reſpirable air of the apartment, and prevents the danger of currents of cold air attracted by the fire; and which is neceſſary, as I have obſerved, whenever the volume of air in the apartment is not ſufficient, to ſupply both the conſumption of the fire, and the circulation in the heat pipes.

Fig. 2. is a plan of the foundation of the hearth at the height of the line A B, fig. 1. *l l* are empty ſpaces, to receive the air, and convey it into the compartments, where it is to be heated before it iſſues by the heat openings, whether the air be obtained from without, or ſimply by the apertures *m m*, fig. 1.

Fig. 3. pl. IV. plan at the height of the line C D, fig. 1; that above the door of the fire-place, *n n n n* are the double plates of caſt iron, forming the compartments in which the air is to receive the effect of the heat of the fire,

o o The empty ſpace between theſe plates.

Fig. 4. Front ſection at the line I K, fig. 3. The arrows indicate the direction of the ſmoke in the circulatory pipes of the front part.*

In this the plates of iron *n n* are ſeen in their perpendicular ſituation, with the tongues which form their compartments on each ſide of the fire-place. One of theſe plates is repreſented in front fig. 7.

T is an opening left at the bottom of the fourth circulatory pipe, to reſtore the draught of air in the fire-place, if neceſ-

* Among the number of Swediſh ſtoves deſcribed and delineated in the collection publiſhed by baron Cronſtedt there are ſeveral, the circulating pipes of which paſs under the hearth. This gives them a little more extent no doubt, but as ſoon as the hearth is covered with aſhes, the air paſſing beneath can receive but a very ſlight impreſſion of heat; it obliges the fire-place to be raiſed higher; and it renders the conſtruction more complex and expenſive. For theſe reaſons I have adopted the moſt ſimple plan.

ſary, by burning there a few ſlips of paper, or other light combuſtible, I ſay *if neceſſary,* becauſe I have found by experience, that this precaution may be neglected, as ſoon as the ſtove has been heated ſo as to have loſt all its internal dampneſs.

The door of this ſort of blower, or air-vent, ought to ſhut very cloſe. For this purpoſe it is ſufficient, to cut a piece of brick of the proper ſize, to make a hole in it to receive a handle, and to faſten upon it a piece of plate iron projecting a little all round it.

Fig. 5. Plan at the height of the line E F, fig. 1.

Fig. 6. Tranſverſe ſection at the line G H of fig. 3, which ſhews the height of the fire-place, and the firſt direction of the flame.

V points out the arrangement of the heat pipes.

The dotted lines give the profile of the party walls, which form the four grand circulating pipes.

R the pipe which conveys the ſmoke from the circulatory pipes into the chimney, and in which is the regiſter that cuts off the communication. It is a common ſtove tunnel of plate iron; but it would be better to uſe a ſubſtance more ſlowly conducting heat, as an earthen tube made on purpoſe, for that part in which the ſlider or ſtop plate acts.

The elbow made by this pipe to reach the chimney renders it unneceſſary to repeat, that it is a point of the firſt importance for the body of the ſtove to be completely ſeparate from the wall. That which I have deſcribed is 25 centimetres (about 9 inches) diſtant from the neareſt point of the nich in which it is placed.

S is an elongation of the perpendicular pipe that enters into the chimney. It is intended to receive the water that might condenſe in the upper part, to prevent it from getting into the ſtove. The cap at the end of this elongation allows the pipe to be cleaned without taking it down.

The dotted lines forming the ſquare ſpace Q mark a place where a nich may be made, or a ſort of little ſtove, as is done in ſome of the Swediſh ſtoves, and would advantageouſly ſupply the place of the brick-work, with which it muſt otherwiſe be filled up.

All theſe figures being drawn on the ſame ſcale, there will be no difficulty in preſerving the proportions of the parts.

The conſtruction of this ſtove is neither difficult nor expenſive. For the outſide nothing is wanted but Dutch tiles, ſuch as are uſed for common ſtoves, that is to ſay thin in the middle, and having a border all round, which ſerves to give them more ſtability. They are fixed in like manner by a band of metal. The hind part may conſiſt entirely of bricks. The vaſe placed on the ſlab of marble or ſtone, which covers the ſtove is a mere ornament.

Materials of which the ſtove is conſtructed.

If it be thought proper to have no heat openings, all the interior ſtructure may be made of bricks of proper ſizes, laid with loamy earth moiſtened, and ſet on edge for the circulatory pipes, without any iron except a caſt plate over the fireplace, and a door and frame in the uſual manner.

The heat openings may be omitted.

The expence of the heat openings however conſiſts only in four caſt iron plates with tongues and grooves to form the compartments repreſented at fig. 7. All the reſt is done with plate iron, bent round and rivetted, which, when once encloſed in the maſonry, will not admit the eſcape of the air.

The expence however not great.

How are they made.

Caſt iron plates with grooves are well known, ſince Franklin's ſtoves have been adopted. If it were found difficult to procure them, their place might be ſupplied in two ways. Firſt by portions of pipes of caſt iron, which might be placed vertically ſide by ſide, ſerving as the inſide walls of the fireplace, and communicating with each other by little channels at top and bottom formed in the maſonry. Secondly, by common plain caſt plates, ſoft enough to admit of being bored, ſo as to rivet on bent ſlips of plate iron, which would perfectly anſwer the purpoſe of the tongues and grooves. As theſe would never be expoſed to the action of the flame, there is no reaſon to fear their caſting. The latter of theſe two methods is obviouſly the moſt advantageous, as it occupies leſs room, and yet affords more ſurface to receive the action of the heat, and communicate it to the circulating air.

Subſtitutes for the caſt-iron plates.

In concluding my deſcription of this ſtove I ought not to omit ſaying, that nearly two years experience has convinced me of the good effects of its proportions.

Its utility confirmed by two years experience.

It is placed in a room fronting the north, the floor of which meaſures 47 metres ſquare (about 12 toiſes $\frac{1}{2}$) and which is 42.5 decimetres (13 feet) high.

Account of its effects.

Every day a log of wood 28 or 30 centimetres (10 or 11 inches) round, ſawn into three pieces, or an equal quantity of ſmaller wood, is burned in it at once. The ſlider of the door

of

of the fire-place is ſhut, and the key R, fig. 6 is turned, as ſoon as the wood is reduced to charcoal. Ten hours after the air throughout the room is at a temperature above the mean; and the centigrade thermometer, placed 36 centimetres (above 13 inches) from the ſtove, riſes rapidly to 16 or 17 degrees.

To ſhew ſtill more plainly to what degree the economy of fuel and preſervation of heat may be carried by this conſtruction, I ſhall relate another experiment, which I have repeated on ſeveral occaſions, and which has always afforded me very nearly the ſame reſults.

The thermometer in the room, in which there was no fire the day before, being between 9 and 10 degrees, a log ſawn in three as uſual was put into the fire-place about eleven in the morning; and at three in the afternoon a ſimilar quantity of fuel was put in.

At four o'clock the thermometer, placed at the diſtance above-mentioned, was at 42 degrees.

At five, at	37 degrees.
At ſeven,	34.
At nine,	31.
At midnight,	26.

You could not bear to touch with the hand the iron rim of the heat openings. The bulb of the thermometer being placed oppoſite one of theſe openings, at the diſtance of 8 centimetres (about 3 inches) roſe in four minutes to 35°.

The next morning at 9 o'clock the thermometer, which had been again placed at the diſtance of 35 centimetres, was at 22°.

Finally at noon, that is to ſay twenty-one hours after the laſt wood was put in, and eighteen hours after the key had been turned, all the wood being reduced to charcoal, the thermometer ſtood between 18° and 19°. It was then placed two centimetres only from one of the heat openings, and in leſs than ſix minutes it roſe to 26°.

Theſe effects are ſo different from what we commonly obtain by the conſumption of three or four times as much fuel, that I may expect more than one reader to ſuppoſe them exaggerated; but I hope a ſufficient number will be found diſpoſed to make a trial of theſe ſtoves, that their teſtimony and example may at length triumph over our habits, and produce a general conviction, that, without ſuffering any privation ourſelves, we may preſerve for our offspring what uſeleſs waſte is daily robbing them of in an article of the firſt neceſſity.

VII. *A Con-*

VII.

A Continuation of the Experiments and Obſervations on the Light which is ſpontaneouſly emitted from various Bodies; with ſome Experiments and Obſervations on ſolar Light, when imbibed by* CANTON's *Phoſphorus. By* NATHANIEL HULME, *M. D. F. R. S. and A. S.*

SECTION XI.

The Effects of various aerial Fluids on ſpontaneous Light.

INTRODUCTION.

THE apparatus employed for experiments with any kind of air, unleſs otherwiſe expreſſed, conſiſted of the following parts: 1. A tea-ſaucer, holding about three ounces of water. 2. A wide-mouthed phial, which would contain about ten ounces of liquid. 3. A ſmall wooden ſtand, compoſed of a ſlender pillar or pin, nearly four inches high, fixed into a round baſe, a little more than an inch in diameter, and half an inch thick. This ſtand was faſtened by ſtrong thread to the middle of a piece of flat lead, ſuch as lines Chineſe tea-cheſts, having holes in it to admit the thread; the lead was about three inches ſquare, and doubled, to give it weight and ſtability. The top of the pillar was made pointed; and a round piece of cork, about an inch in diameter, and half an inch thick, was fixed upon it, by means of a ſuperficial hole bored in its under part with a gimlet. Apparatus made uſe of,

When the whole apparatus was put in uſe, the phial was filled with cold pump water, in a pneumatic tub, then inverted, and the ſpecies of air to be employed was let up into it, to the quantity of about eight ounces. The ſubject for experiment being applied to, or faſtened upon, the top of the cork, the ſtand was placed on the tea-ſaucer, and then introduced, under water, into the phial containing the air. The whole apparatus, being now ſupported by the tea-ſaucer, and its uſe.

* Phil. Tranſ. 1801. p. 483. The former part is in the Phil. Tranſ. for 1800, page 161, and alſo in the Philoſ. Journal, Quarto, IV. 421, 451.

with

with water in it, was depofited in the laboratory for experiments on light. By this contrivance, the experiments were made in about eight ounces of air, by meafure, confined above two ounces of water.

§ 1.

The Effects of common or atmofpherical Air on fpontaneous Light.

EXPERIMENTS.

The exclufion of atmofpheric air prevents the appearance of fpontaneous light.

Experiment 1. Two frefh herrings were hung up together in the laboratory, fo as to touch each other at their flat fides; and it was obferved that the parts in contact remained dark, while thofe expofed to the open air became very luminous.

Fifh.

Exp. 2. Another frefh herring was laid upon a piece of thick brown paper, and placed in the laboratory. On examination, the next evening, the upper part, which was expofed to the air, was very lucid; but the underfide, lying upon the paper, remained quite dark.

Exp. 3. A luminous herring was divided tranfverfely quite through its middle flefhy part; but the infide was perfectly dark. On the following night, that which before was dark had become luminous.

Exp. 4. At 9 P. M. a piece of frefh herring, of about three drams in weight, was introduced above water, into about eight ounces of atmofpherical air. On the fecond night it was become luminous; on the third and fourth, it continued fhining; and on the fifth the light was extinguifhed. This experiment was frequently repeated, with both the flefh of herring and of mackerel, and nearly with the fame refult.

Exp. 5. The cork of the apparatus was well fmeared with the luminous matter of a mackerel, and then introduced above water. It continued to fhine finely all that evening; and the light was not quite extinct on the fucceeding night.

Exp. 6. Another cork was illuminated with herring-light, at half an hour paft fix P. M. and introduced above water. It remained very bright at eleven; and retained a glimmering light the next evening. The two laft experiments were often repeated, and, in general, with fimilar effects. It may not, however,

however, be improper to obſerve, that the illumination of the cork did not always continue ſo long as twenty-four hours; for it muſt, of courſe, vary according to the quantity of luminous matter applied, and its degree of brilliancy.

Exp. 7. A large piece of rotten wood was received from the country, which ſhone only in one place. The luminous portion was ſawed off for uſe, and the dark part left in the laboratory. On going into the laboratory, the ſecond night after this operation, I was ſurpriſed to ſee the dark piece, which had been left there, very lucid in ſeveral places where ſmall ſplinters had been broken off in ſawing; many ſhining fragments alſo lay ſcattered on the floor. **Rotten wood.**

Exp. 8. A quantity of rotten wood, moderately ſhining, was blown upon for ſome time with a pair of bellows; but I could not perceive that this had any effect on the light, ſo as to render it more vivid.

Exp. 9. A ſmall piece of ſhining wood was tied upon one of the corks of the apparatus, and introduced above water, where it continued lucid until the fifth night. In another experiment, the light was extinguiſhed on the fourth night; and and in a third much ſooner.

Exp. 10. A living glow-worm, in a ſhining ſtate, was ſubmitted to the action of a pair of bellows; but the continuance of the blaſt did not apparently increaſe its glowing quality. **Glow-worm. Theſe are not excited by bellows.**

Exp. 11. A very luminous dead glow-worm was fixed upon a cork of the apparatus, by means of a ſmall pin, and then put into the phial, above water. It continued to ſhine as vividly as it did when in the open air, forming a pure white light, of a circular ſhape.

OBSERVATIONS.

Obſervation. 1. Theſe experiments prove, that objects which abound with ſpontaneous light in a latent ſtate, ſuch as the herring, mackerel, and the like, do not emit it when deprived of life, except from ſuch parts as have been ſome time in contact with the air.

Obſ. 2. They likewiſe ſhow, that the blaſt of a pair of bellows does not increaſe this ſpecies of light, as it does that which proceeds from combuſtion.

 The

§ 2.

The Effects of oxigen Gas or vital Air on spontaneous Light.*

EXPERIMENTS.

Fish, shining wood and Glow-worms in common air and in oxigen gas exhibited no difference of illumination.

Experiment 1. A piece of fresh herring, of about three drams weight, was introduced above water, into eight ounces of oxigen gas. On the second night it was observed to be faintly luminous; on the third, the quantity of light was increased; on the fourth, it continued nearly in the same state; and on the fifth the light was diminished.

Exp. 2. A piece of very fresh mackerel, of the same magnitude, was also put above water. On the subsequent evening it was pretty lucid, and continued the same on the night following.

Exp. 3. At 9 P. M. a cork, finely illuminated with mackerel-light, was introduced above water: it continued very lucid at eleven. On the next evening it was dark.

Exp. 4. Another cork, rendered luminous with the same kind of light, was put above water at 10 P. M. The next morning, at six o'clock, only a glimmer of light was perceived and at 10 P. M. it was extinct.

Exp. 5. At 9 P. M. a fragment of shining wood was introduced above water; it was observed to be still very luminous at eleven; but the light was not quite so vivid, nor so extended in breadth, as when the wood was put in. On the succeeding night, at eight o'clock, it remained faintly lucid.

Exp. 6. A little after 8 P. M. another fragment of wood, shining very brightly, was introduced above water, into the same air that was used in the last experiment: it continued very luminous at eleven; but the light was diminished in quantity. On the next evening it was found to be extinguished.

Exp. 7. The same air was employed again at 8 P. M. with a pretty large and thick fragment of wood, uncommonly lucid: its light continued vivid and broad at half an hour past eleven. The following night, at eight o'clock, the light was still somewhat extensive and bright.

Exp. 8. In three other experiments with shining wood, in fresh oxigen gas, the light was totally extinguished in the space of twenty-four hours.

* The oxigen gas made use of was obtained from manganese, by means of heat

Experiments were made, at the fame time, and in the fame manner, with atmofpherical air and fhining wood; but it was not very evident that the wood fhone more vividly in the latter air than it did in the oxigen gas.

Exp. 9. A living glow-worm was put into a two-ounce phial, with a glafs ftopple, containing pure oxigen gas, and kept therein for fome time. It was then taken out, and expofed to the open air; but no difference, either in the brilliancy or the quantity of its light, could be difcovered.

Exp. 10. A luminous dead glow-worm was then inclofed in about five ounces of the gas; but no increafe of its fhining quality could be perceived.

Exp. 11. At fix o'clock P. M. a fhining dead glow-worm was introduced above water into oxigen gas: it continued very lucid therein at 7 P. M. fhewing a pure white light. It was then taken out, and put above water into atmofpherical air, where it fhone, to all appearance, as fplendidly as it did when it was in the oxigen gas.

OBSERVATION.

It appears, from thefe experiments, that oxigen gas does not act upon this kind of light, fo as to render it much more vivid than it is in atmofpherical air; which is quite contrary to what fome authors have alledged.

§ 3

The Effects of azotic Gas on fpontaneous Light.

1. *Azotic Gas, obtained from lean mufcular Flefh and diluted nitric Acid, in a very low Heat, as recommended by M. de* FOURCROY.

EXPERIMENTS.

Experiment 1. A piece of frefh mackerel, weighing about three drams, was introduced above water, into about eight ounces of this azotic gas; and it was retained therein five days, without emitting any light.

Azote gas does not admit of the production of fpontaneous light but maintains it in fome inftances for a time if previoufly produced.

Exp. 2. About the fame quantity of frefh herring was then put above water, into the fame gas ufed for the laft experiment, and remained in it for the fpace of three days, in a dark ftate. This experiment was repeated, and with a fimilar refult.

Exp. 3. At 45 minutes paft 7 P. M. a cork, finely illuminated with mackerel-light, was put above water into the gas, and it was found pretty luminous at eleven. On the next evening, at eight o'clock, it ftill exhibited a faint degree of light.

 A fimilar

A ſimilar experiment was made, at the ſame time, in atmoſpherical air. At 11 P. M. the cork was but moderately luminous; and on the next evening it was dark.

Exp. 4. At 40 minutes paſt 7 P. M. another cork, rendered very luminous with herring-light, was introduced above water. This cork, at 11 P. M. was not found ſo lucid as that in the third experiment. On the next evening, a glimmer of light was ſtill perceptible.

Exp. 5. A fragment of very ſhining wood was introduced above water, into this gas; and it was rendered dark in about 15 minutes.

Exp. 6. The experiment was repeated; and the light was again extinguiſhed in about 15 minutes. In another experiment it was extinguiſhed in about 25 minutes.

II. *Atmoſpheric Air rendered azotic, by burning Spirit of Wine in it, when confined above Water*

Atmoſpheric azote.

Exp. 7. A portion of freſh herring, of about three drams, was put above water, into this azotic gas, at 5 P. M. On the ſecond evening, a ſpark of light was obſervable; on the third, the quantity of light was increaſed; on the fourth, it was again diminiſhed.

Exp. 8. At 3 P. M. the uſual quantity of herring was introduced above water. On the ſecond night, it remained dark; on the third it was moderately luminous; on the fourth, it was leſs ſo; on the fifth, the light was extinct.

Exp. 9. A piece of freſh mackerel was next put above water, at 11 A. M. On the ſecond evening, it was found to be ſlightly luminous; it remained ſo on the third; on the fourth, it was dark.

Exp. 10. Another piece of freſh mackerel was introduced above water, at 3 P. M. On the ſecond night, it was found to be ſlightly luminous; but on the third, it was dark; and no more light was emitted, though it was kept in the gas for the ſpace of four days.

Exp. 11. A cork, made very luminous with herring-light, was put above water, into this gas, at 20 minutes paſt 8 P. M. and it continued very lucid at eleven. The next evening, at ten o'clock, the light was nearly extinguiſhed.

A ſimilar experiment was made, at the ſame time, in common atmoſpherical air, and with the ſame reſult.

Exp.

Exp. 12. Another cork was introduced above water, with herring-light, at 40 minutes paſt 7 P. M. and it remained pretty luminous at eleven. On the following night, it was nearly extinct.

III. *The laſt mentioned azotic Gas, after being waſhed with Lime Water.*

Exp. 13. A piece of herring, of about three drams weight, was put above water, into this azotic gas, at 5 P. M. On the ſecond night, it was dark; on the third, very lucid; and on the fourth, the ſame.

Exp. 14. The experiment was repeated, on a piece of herring, at 3 P. M. On the ſecond evening, it was dark; on the third, pretty luminous; on the fourth, it was leſs ſo; and on the fifth, only a faint light remained.

Exp. 15. A portion of freſh mackerel was then put above water, at 11 A. M. On the ſecond night, it was obſerved to be moderately ſhining; on the third, the light was extinct.

Exp. 16. Another piece of freſh mackerel was introduced above water, at 3 P. M. On the ſecond evening, it was ſlightly luminous; on the third, it was dark, and continued ſo during the four ſucceeding nights.

Exp. 17. A cork, finely illuminated with herring-light, was next introduced above water, into this gas, at 20 minutes paſt 8 P. M. The light was much diminiſhed at 45 minutes paſt 8; at 11 the cork had become almoſt dark. On the following night, a glimmer was ſtill apparent.

Exp. 18. Another cork, made very luminous with herring-light, was put above water, at 40 minutes paſt 7 P. M. and it continued pretty lucid at eleven. On the next evening, the light was merely viſible.

A ſimilar experiment was made, at the ſame time, in atmoſpheric air, and with nearly the ſame effect.

OBSERVATION.

It is a remarkable circumſtance, that azotic gas, which is incapable of ſupporting light from combuſtion, ſhould be ſo favourable to the ſpontaneous light which is emitted from fiſhes, as to preſerve its exiſtence and brilliancy for ſome time, *when applied upon a cork;* yet that it ſhould prevent the *fleſh* of the herring and the mackerel from becoming luminous, and alſo extinguiſh the light proceeding from rotten wood.

The

§ 4

The Effects of hidrogen Gas or inflammable Air on spontaneous Light.*

EXPERIMENTS.

Hidrogen gas does not admit the production of light.

Experiment 1. At 9 P. M. a piece of fresh herring, weighing about three drams, was introduced above water, into hidrogen gas. It was retained therein three days and three nights, without emitting any light. It was then taken out, and exposed to the action of atmospherical air. On the following night it was found to be luminous; but was dark again on the next night.

Exp. 2. Another piece of fresh herring was put above water, at 6 P. M. This was also kept in the gas the same length of time, without producing any light. It was then exposed to the open air, and inspected two successive nights, but it remained dark.

Exp. 3. The same experiment was then made with a piece of mackerel, which was taken out on the fourth night, without producing any shining appearance. The next evening, it emitted a very faint light, which did not continue twenty-four hours.

but extinguishes it. (Fish)

Exp. 4. A cork, brilliantly illuminated with mackerel-light, was introduced above water; and the light was extinguished in about the space of an hour.

Exp. 5. At 39 minutes past 9 P. M. another luminous cork was put above water; it lost some of its light pretty soon, but was not extinct at twelve.

Exp. 6. A cork, with herring-light, was introduced above water, at 23 minutes past 6 P. M. The light gradually diminished, and was only faintly visible at eleven.

Exp. 7. A fragment of very shining wood was put above water, at 9 P. M. and was dark at eleven.

Common air again revives it.

Exp. 8. Another fragment was put above water, at 40 minutes past 8 P. M. at 50 the light was much diminished, and at 8 minutes past 9 the shining ceased. The wood was then taken out, and exposed to the open air, when the light revived in a very beautiful manner.

* This gas was obtained from zinc and diluted sulphuric acid.

Exp.

Exp. 9. A piece of uncommonly ſhining wood was introduced above water, at 58 minutes paſt 8 P. M. it remained for a ſhort time very luminous, but at 25 minutes paſt 9 the light was greatly diminiſhed; at 20 paſt 10 it was nearly extinguiſhed; and at 29 paſt 10 was quite dark. It was then expoſed to atmoſpheric air, and the light revived very brightly.

Repetition of extinction and reſuſcitation. (Wood)

Exp. 10. The ſame experiment was repeated, at 35 minutes paſt 8 P. M. the ſhining property was much diminiſhed at 9; and at 10 it was very faint. The next evening, it continued merely viſible. The wood was now taken out, and the light ſoon revived very ſtrongly. The following night, it was ſtill moderately lucid; but on the next evening nearly extinct.

Exp. 11. Finding, by the above experiments, that the light of ſhining wood was extinguiſhed by this ſpecies of gas, and reſtored by atmoſpheric air, the following three trials were made, to diſcover, in ſome degree, how long its light might be kept in a latent ſtate, and then be revived. At 9 P. M. ſeveral fragments of ſhining wood, tied up in a piece of gauze, were introduced above water, into the hidrogen gas, and the light was gradually extinguiſhed during that evening. They were kept there in that dark ſtate 48 hours, were then taken out, and expoſed to the open air, when, after a little time, the light re-appeared.

Extraction for 48 hours.

Exp. 12. On the 2d of October, another fragment of exceedingly ſhining wood, two inches and a half long, and pretty thick, was put above water in the evening, and its light was gradually extinguiſhed. On the ſecond night, it was taken out perfectly dark, but its light recovered by degrees, and became brilliant. It was introduced again, that evening, into the ſame gas, and its light diſappeared. On the third night, it was again expoſed to the open air, and the light revived as before. It was then reinſtated and extinguiſhed, and continued in a dark ſtate, from the third to the fifth night, when, being again taken out, it ſoon ſhone in a pretty vivid manner. It was again introduced and extinguiſhed as uſual; and no obſervation was made of it, from ſome accidental circumſtance or other, until the 10th of November in the evening, when it was taken out, and expoſed to the open air for a length of time, but the light did not revive.

Three repetitions of extinction and renovation.

Exp.

No renovation after a week

Exp. 13. A third fragment, ſomewhat larger than the former, and equally luminous, was put above water, at the ſame time as the one in the laſt experiment, where it was ſoon deprived of its light. It was retained there, in a dark ſtate, from the 2d of October till the 10th of November; it was then taken out, and expoſed to the action of atmoſpherical air, for ſeveral days, but there was no return of light.

Three extinctions and renovations of a glow-worm

Exp. 14. About 7 P. M. a ſhining dead glow-worm was introduced above water into the gas, and its light was ſoon extinct. It was then expoſed to the open air, where, in a very ſhort time, it ſhone as brightly as before.

Exp. 15. At half an hour paſt 9 P. M. the ſame glow-worm was again introduced above water; when its light in a ſhort time diſappeared. It was taken out for expoſure to common air at 11, and its glowing property was immediately reſtored. It was again replaced in the gas, where it ſoon loſt all its light a ſecond time, and was kept in that dark ſtate for 24 hours; when taken out, it continued dark for a little time, and then the inſect gradually recovered its priſtine ſplendour.

OBSERVATION.

From theſe experiments we learn, that hidrogen gas, in general, prevents the emiſſion of ſpontaneous light, and alſo extinguiſhes it when emitted; but, at the ſame time, it does not hinder its quick revival, when the ſubject of the experiment is again expoſed to the action of atmoſpherical air; although the light may have been a conſiderable time in an extinguiſhed ſtate.

(To be concluded in our next.)

Caution

VIII.

Caution againſt the great Danger of keeping Phoſphorus in Bottles without particular Caution. By DESCROZILLES *the Elder.**

I have narrowly eſcaped falling a victim to an accident occaſioned by the effect of the froſt upon a bottle, which contained a hectogramme of phoſphorus, with a quantity of water ſufficient for covering this highly inflamable ſubſtance. This morning, before day-light, ſome books and a cheſt ſuddenly caught fire in the apartment in which I lay, and which is not occupied during the night. It was ſtill fortunate that the two hours of my accuſtomed ſleep were long ſince elapſed. Some ſeconds later, I ſhould have been ſuffocated by the deleterious vapours of the phoſphoric acid. Quickly gaining the door, I called for aſſiſtance, and we ſucceeded in extinguiſhing the fire before it had made any progreſs. Dangerous accident of fire from phoſphorus.

According to all appearance, the froſt which had taken place ſome days before in this piece, had cauſed the bottle to break; but it was ſurrounded with paper, which prevented its falling to pieces. By this means, in proportion as the water ran off by the effect of the thaw, the bundle of phoſphorus, expoſed to the atmoſpheric air, was ſituated under circumſtances the moſt favourable to ignition. The ſides of the bottle performed the office of a ſmall furnace, in which the cylinders of the combuſtible were propped againſt one another. Soon the gradual combuſtion which produces phoſphorus acid was ſucceeded by the rapid deflagration, the reſult of which is phoſphoric acid. How it happened.

Independently of the effects of froſt and of blows, a bottle frequently breaks without any apparent cauſe, and as it were ſpontaneouſly: It appears therefore to me, that in order to obviate an inconvenience which in ſome caſes may prove very ſerious, the beſt means would be to uſe caſes of copper, ſtrongly ſoldered, and lined internally with paper or bran, for incloſing the bottles filled with phoſphorus and water. Caſes of tinned iron would be deſtroyed much ſooner by oxidation, and their ſoldering would be ſuſceptible of detaching itſelf by the effect of a moderate heat. Other cautions.

Finally, it appears to me that this report ought to be as public as poſſible.

* In a letter to the Editors of the Annales de Chimie, No. 123.

IX. IN

IX.

Obſervations in Anſwer to DR. PRIESTLEY'S *Memoir in Defence of the Doctrine of Phlogiſton* *. *In a Letter from* MR. WILLIAM CRUICKSHANK.

To Mr. NICHOLSON.

SIR, *Woolwich, March* 22, 1802.

Obſervations of Dr. Prieſtly, that metallic calces contain little addition except water.

IN your Journal publiſhed the *1ſt of this month*, I find a letter from Dr. Prieſtley, in which he ſtill defends the old doctrine of phlogiſton, but with very little ſucceſs; for having additional difficulties to ſtruggle with, he has been under the neceſſity of adopting new, and ſometimes contradictory opinions, in his explanations and defence; For example, it is now his opinion that all metallic calces contain water and little or nothing elſe; and that charcoal, uniting with water, forms both fixed and inflammable air; for as this ſubſtance contains the elements of both kinds, nothing but water is wanted to enable them to take the form of air. It is almoſt impoſſible to argue againſt ſuch ſtrange ſuppoſitious and looſe reaſonings as theſe. However, let us ſuppoſe water to be the only ſubſtance contained in oxides; if ſo, heat alone ought to revive at leaſt ſome of them, and in this caſe nothing but water ſhould be ſeparated. Now that heat alone revives ſeveral, particularly thoſe of mercury and the perfect metals, is a fact ſufficiently aſcertained; but, inſtead of water, we obtain the pureſt oxigen gas, the quantity of which added to the revived metal, amounts, as nearly as poſſible, to the weight of the original oxide; even from the ore of mangaueſe, ſo difficult to reduce, a prodigious quantity of the pureſt oxigen gas may be procured by a moderate heat; and when the oxide has been previouſly reduced to powder and well dried, by a heat nearly red, no water whatever can be perceived: in this caſe, too, the quantity of the oxigen gas diſengaged, will be found to correſpond very nearly with the loſs of weight in the oxide.

But when reduced by mere heat they afford pure oxigen, very nearly equal to their loſs of weight, and no water.

If calces contain water only, then charcoal muſt produce the ſame

How is it poſſible, then, that theſe phenomena can be explained on the ſuppoſition, "That all the calces contain little "or

* Philoſ. Journal I. 181.

"or nothing elſe but water?" when not a particle of water is to be ſeen during the revival, or partial revival, of the metals from their oxides in cloſe veſſels. Let us, however, conſider this ſuppoſition in another point of view; which is, that if water only were contained in theſe oxides, then the gas obtained from a mixture of them and charcoal, ſhould be the very ſame as that procured from moiſtened charcoal; but the contrary of this I have clearly proved to be the caſe (ſee Phil. Journal, vol. v. quarto, p. 6.); and this was one of the principal faults which led to the diſcovery of the gaſeous oxide.

thing by heat with calx as with water;

contrary to fact.

I ſhall here juſt enumerate a few of the properties by which theſe gaſes may be readily diſtinguiſhed, being deduced from a number of experiments, often repeated with uniform reſults. Firſt, then, the ſpecific gravity of the gaſeous oxide of carbon is no leſs than double that of the gas obtained from moiſtened charcoal, being as 30 to 14.5. Secondly, The proportion of oxigen neceſſary to ſaturate the gaſeous oxide, is to that required by the hydrocarbonate as 15 to 44.8, or 1 to 3, nearly eſtimating by quantity. And, thirdly, which is the moſt diſtinguiſhing property of the whole, the ſame quantity of oxigen, ſuppoſe 14 parts, which, when combined with the gaſeous oxide of carbon, produces from 36 to 44 parts of carbonic acid (according to the purity of the oxide), will, when combined to ſaturation with the hydrocarbonate, produce only 12 parts of the ſame acid, accompanied, however, with much water, proceeding in part from the hidrogen in the charcoal, of which all common charcoal, it would appear, contain a certain proportion (ſee P. Jour. No. 55. p. 210 and 211, and alſo the Table of Analyſis, &c. p. 8. No. 59.) Surely gaſes having properties ſo eſſentially different, can never be conſidered as the ſame.

Enumeration of differences; ſpecific gravity of gaſeous oxide of carbon double that of hydrocarbonate;

ſaturating oxigen very different;

and products of carbonic acid with like portions of oxigen.

Dr. Prieſtley remarks, that there is a conſiderable difference in the qualities of heavy inflammable air, depending not only on the ſubſtance employed, but alſo on the ſtages or periods of the proceſs itſelf*. Now I have found that theſe variations never take place in any remarkable degree but when charcoal is employed in ſome form, and that even then the differences chiefly depend upon the imperfect ſtate of the charcoal; for if good charcoal be expoſed to a red heat in cloſe veſſels during 10 or 15 minutes, and then mixed whilſt hot with the ſubſtance

Differences in heavy inflammable air noticed by Dr. Prieſtley,

are found only when charcoal is preſent, and more or leſs impure;

* P. Journ. No. 3. (Oct.) p. 183.

to

to be employed, likewiſe hot and completely dried, the variations in the qualities of the gaſes will be much leſs, the principal difference depending upon the proportions of the carbonic acid and the inflammable gaſes; but the gaſes from pure charcoal, either alone or moiſtened, never have any of the diſtinguiſhing characters of the gaſeous oxide, being always much lighter, yielding bulk for bulk when ſaturated with oxigen, not more than one-third of the carbonic acid gas afforded by the other. What, however, diſtinguiſhes them ſtill more completely, is the large proportion of water generated by the combuſtion of the hydrocarbonate in oxigen gas; for the gaſeous oxide when pure, or burned under the ſame circumſtances, never produces the leaſt ſenſible quantity of this fluid.

the hydrocarbonates are much lighter than the gaſeous oxide, and they afford water by combuſtion with oxigen.

Erroneous quotation and remarks.

In mentioning the circumſtances of the production of the gaſeous oxide of carbon from the metallic calces and charcoal, &c. Dr. Prieſtley has ſomehow mis-ſtated both the meaning and words in what he calls a quotation from the firſt paper; this paſſage in his letter is as follows (ſee p. 182.): "After "repeating my experiment, which he found to be juſt, Mr. "Cruickſhank did the ſame with the calces of the other me"tals, as zinc, copper, &c." and then concludes (p. 4.) "*that* "*in* ALL *theſe caſes the air muſt come from the partial decompo*"*ſition of the carbonic acid by the calx, when raiſed to a high* "*temperature*:" Then the Dr. goes on and adds: "But the "inference that I think is more naturally drawn from them is, "that all theſe calces contain much water, and little or no"thing elſe." Now the paſſage from which this quotation appears to have been taken (for there is nothing in p. 4. exactly ſimilar to it), does not follow the account of the experiments with the calces of the metals, &c. but is an inference drawn from the firſt experiments made with the carbonates and the iron ſcales. I ſhall inſert this paſſage, and leave the philoſophical reader to judge for himſelf of the accuracy of the ſtatement and the juſtneſs of the concluſion. "Conceiving "that in theſe experiments * the gaſeous oxide muſt proceed "from the partial decompoſition of the carbonic acid by the "iron when raiſed to a high temperature, I thought I ſhould "ſucceed better by employing iron filings in place of the grey

* Alluding to theſe made with the carbonate of barytes and iron ſcales, or imperfect grey oxide.

" oxide, as thefe would have a greater affinity for oxigen." From the above ftatement the mifreprefentation both of the explanation and words muft be manifeft, and the infertion of the word *all* is curious enough. (Effay on Gafeous Oxide, &c. Philos. Journal, vol. v. quarto, p. 4.)

In the experiments made with the mixtures of carbonate of lime dried as much as poffible and metallic filings, the Dr. conceives that the gas muft have been produced by the water which, ftill remaining in the carbonate, had united with the phlogifton of the metal, and paffed over in the form of inflammable gas, mixed with the fixed air feparated from the chalk.

Filings and chalk heated, fuppofed by Dr. Prieftley to afford a gas confifting of phlogifton and water.

If the inflammable gafes were produced in thefe cafes by the water feparating from the earth, and paffing over the red-hot metal in the form of fteam, then they fhould be the very fame as that obtained by paffing the vapour of water through a red-hot iron tube; but it is well known, the gafes thus produced are fo far from being the fame, that they are extremely different in all their properties; for the hidrogen, or light inflammable air, produced by the decompofition of the water in paffing through the tube, is the lighteft of all aeriform fluids, and, when combined with oxigen, there is not the leaft appearance of carbonic acid, and nothing formed but water. On the contrary, the gafeous oxide procured by heat from a mixture of the drieft earthy carbonates with metallic filings, is the heavieft of all known inflammable gafes, and when united with oxigen, produces nothing but carbonic acid, there not being the leaft appearance of water. It is impoffible, therefore, that thefe heavy inflammable gafes fhould be produced by water alone, in any ftate, acting upon pure metals; for the gafes obtained from the decompofition of this fluid, whether from the folution of metals in dilute acids, or from their action on its vapour when raifed to a red heat, are always of the fame nature, being pure hidrogenous gas.

If fo, the gas ought to be the fame as with filings and water; but this is the lighteft of elaftic fluids, and *forms water* by combuftion; whereas the gafeous oxide is the heavieft inflammable gas, and produces carbonic gas by combuftion, *with no water.*

Dr. Prieftley obferves, that before I admitted that the iron or its calx, raifed to a high temperature, could decompofe the carbonic acid in this experiment, I fhould have tried whether it would do it in any other. This remark clearly proves that he had not feen the fecond effay on the gafeous oxide, &c. in your Journal for the month of Auguft, in which a procefs is defcribed, where this acid is decompofed, even in its gafeous form, by paffing it repeatedly through a red-hot iron tube filled

Recapitulation of the experiment wherein carbonic acid gas paffed over ignited iron gave oxigen to the metal, and was itfelf converted into carbonic oxide.

in

in the middle with iron wire. Bladders filled with this gas were attached, by ſtopcocks, to the extremities of the tube. After the middle of the tube, placed in a furnace, had been made red hot, the gas was repeatedly paſſed, and very ſlowly, from one bladder to the other. During this paſſage it was expoſed to a very extenſive ſurface of red-hot iron; and in one of the laſt experiments made in this manner, after forcing the gas through the tube backwards and forwards 30 times, four parts in five of the carbonic acid gas was converted into gaſeous oxide: the gas upon the whole was a little diminiſhed. The iron wires, after the operation, were covered with the ſame ſhining cruſt as if the ſteam of water had paſſed over them; and in fact they were oxidated or calcined to a certain extent, in conſequence of ſeizing from the carbonic acid a proportion of its oxigen ſufficient to convert it into the intermediate ſtate of an oxide *.

Dr. P. could not decompoſe carbonic acid by the burning glaſs with iron, becauſe the contact of ignited ſurface was inſufficient.

The method by which Dr. Prieſtley attempted to decompoſe the carbonic acid gas, by heating pieces of iron with a lens placed in it, could not poſſibly have ſucceeded, at leaſt to any ſenſible degree; for the heated air when left at liberty to aſcend, would not remain in contact with it, nor even near it, for a moment: thus every portion of air, being unconfined, would expand prodigiouſly, ſo that very little of it could come or remain in contact with the heated metal. In the experiment above related, the gas was computed to paſs ſeveral times over a ſurface of red-hot iron of ſome extent; yet, notwithſtanding this, a conſiderable time was required to produce any remarkable decompoſition in the acid; a circumſtance which proves that, in the method employed by Dr. Prieſtley, no ſenſible decompoſition could have been produced.

Simpler method of decompoſing carbonic acid gas.

There is likewiſe, in my ſecond Eſſay, another method ſtill ſimpler for decompoſing the carbonic acid in its gaſeous ſtate †. All the oxidible metals are, when raiſed to a high temperature, capable of decompoſing it more or leſs; but zinc is by far the moſt powerful, owing no doubt to its greater affinity to oxigen.

Though mere heat does not expel oxigen from finery cinder; yet heat and the attraction of charcoal may;

Dr. Prieſtley aſks, if, in the experiment with the finery cinder and charcoal, the oxigen to form the carbonic acid ſhould come from the calx, how is it to be expelled, as heat alone will not do

* For the particulars of this experiment, and the mode of conducting it, ſee this Journal, quarto ſ. No. 55. p. 209.

† No. 55. p. 209.

do it, &c. &c.? But here he has forgotten that the affinities of bodies are remarkably varied by change of temperature, and that, in all reductions of metallic oxides, the carbon of the charcoal, when raifed to a high temperature, unites with the oxigen of the calx in confequence of increafed affinity, and forms with it carbonic acid gas and gafeous oxide. That this is the cafe is remarkably proved by diftilling the red oxides of mercury and lead with charcoal; for thefe oxides we know contain a large proportion of oxigen, which may be feparated by heat alone, but when heated with charcoal, nothing but carbonic acid and gafeous oxide come over, becaufe, in this cafe, the whole of the oxigen being in its nafcent ftate, combines, even at a low temperature, with the charcoal, and paffes over in the form of carbonic acid gas mixed with the gafeous oxide. There are fome circumftances accompanying the diftillation of charcoal with thefe metallic oxides, which are eafily reduced, and at low temperatures, not altogether uninftructive, and, in my opinion, unanfwerable by the fupporters of phlogifton. In thefe proceffes the proportion of gafeous oxide to carbonic acid gas is but fmall, being in general about one-third or one-fifth of the whole, and for the moft part is obtained juft before or at the time the mixture becomes red: It is always accompanied with a torrent of carbonic acid gas. At the inftant the metal is revived, the gas either ceafes entirely, or comes over very flowly; but on increafing the heat, it again makes its appearance, and is now fo far from containing gafeous oxide, that it is peculiarly light, not mixed with any fenfible quantity of carbonic acid gas, and yields, when faturated with oxigen, but a very fmall proportion of this acid gas.

as it does in all reductions.

Red oxides of mercury and lead afford carbonic acid and oxide.

Circumftances of thefe experiments.

Deductions.

The following facts may be drawn from thefe experiments: Firft, It would appear that a much greater degree of heat is neceffary for the proper production of the gafeous oxide than for the carbonic acid. Secondly, That oxigen, in its nafcent ftate, may unite with carbon at lefs than a red heat, and form carbonic acid; as is clearly proved by the procefs with the red oxide of mercury and charcoal. The gafeous oxide appears likewife to be produced at a very low heat.

Conclufion.

I have now taken a view of all the arguments which Dr. Prieftley has brought forward in defence of his former opinions, but fhall at prefent make no further obfervation on this fubject, leaving the argument and fact to be decided by your philofophical and chemical readers.—I am, SIR, &c.

X.

On the new Planet CERES.

To Mr. NICHOLSON.

SIR, *Parſon's Green, April* 3, 1802.

The ſubject reſumed.

AT the concluſion of the memoir concerning the new planet Ceres, which you did me the honour to publiſh in your laſt number of the Philoſophical Journal, a want of leiſure, and the length of the communication, were alledged as reaſons for my not concluding, at that time, the whole of the obſervations which I had to offer on the ſubject: I beg leave, therefore, now to reſume the examination and detail of thoſe particulars which remain yet to be treated of.

Diſcovery of a method of deducing the greateſt equation from the eccentricity.

About four years ago, when I was inventing a mechanical contrivance, by which the equation of the center and true diſtance of a planet, or any number of planets, might be exhibited in an orrery, I diſcovered that the *natural ſine of half the greateſt equation of any planet, is equal, or very nearly equal, to the decimal figures which repreſent the value of a vulgar fraction, compoſed of the eccentricity and mean diſtance of that planet:* For inſtance, if we take the mean diſtance of the earth from the ſun at 100000, and the eccentricity, according to Lalande, at 1681,395, the fraction $\frac{1681,395}{100000}$, converted into a decimal expreſſion of the ſame value, is .01681395; and, omitting the decimal point and three laſt figures, we ſhall have 01681 for the natural ſine of 0° 57′ 47,6″, which arc differs only about *half a ſecond* from one half of the greateſt equation, as given in the tables of the third edition of Lalande's Aſtronomy.

The proceſs, in the form of an analogy, will be thus: As the mean diſtance : is to unity :: ſo is the eccentricity : to the natural ſine of $\frac{1}{2}$ the greateſt equation.

Tabulated numbers to ſhew its correſpondence in all the planets.

This analogy will apply to all the other planets, as may be ſeen in the ſubjoined table, which I have calculated from the mean diſtances and eccentricities given in Lalande's Aſtronomy, and copied by Mr. Vince, except in the inſtance of Ceres, the data of which planet are taken from the elements of Gauſs.

Planets.

Planets.	Vulgar Fractions.	Decimals, or Nat. Sines.	Correspondent Arcs.	Half the greatest Equation, 1750.
			° ′ ″	° ′ ″
Mercury	$\frac{7955,4}{38710}$	,20551	11 51 34,9	11 50 0
Venus	$\frac{498}{72333,24}$	,00688	0 23 38,3	0 23 40
Earth	$\frac{1681,395}{100000}$	,01681	0 57 47,6	0 57 48,2
Mars	$\frac{14183,7}{152369,27}$	,09308	5 20 26,9	5 20 20
Ceres	$\frac{8250}{276730}$	,02981	1 42 28,96	
Jupiter	$\frac{250137}{520379,2}$	,04807	2 45 18,6	2 45 19,15
Saturn	$\frac{53640,42}{954072,4}$	,05622	3 13 22,7	3 13 21
Georgian	$\frac{90804}{1918352}$	,04733	2 42 45,5	2 43 38

In this table the greatest difference between the arcs contained in the same line of the two last columns, is that in the line of Mercury; but it may be worthy of remark, that half the greatest equation of this planet, according to Dr. Halley's tables, is 11° 51′ 18″; and also that Lalande himself made a new determination of the elements of Mercury's orbit, as related in the "Memoires de l'Institute Nationale" of Paris for the "fourth year of the Republic;" in which the grand equation is given 23° 40′ 45″.

Remark concerning mercury.

Here, then, it appears, that the greatest equation of the new planet Ceres, which corresponds to the eccentricity assigned by Gauss, and copied into the different Journals, is about 3° 25′, instead of 9° 27′ 41″; so that, as I hinted before, either the eccentricity is almost two thirds *too little*, or the greatest equation almost two thirds *too much*. I mean not at present to enter into a geometrical demonstration of the analogy which I have used in procuring the above tabulated results; but leave it to exercise the ingenuity of your mathematical readers, some of whom will probably be induced to favour you and the public with a demonstration, as a separate article. I will, however, just prove to the reader the accuracy of the inference I have made with respect to Ceres, by means of the *elliptic hypothesis* of Ward, which is generally allowed to be a convenient approximation to be used for finding the equation of a planet, instead of either the direct or tentative methods, which are more accurate, but much more intricate.

Hence the greatest equation of Ceres, as assigned, does not correspond with its eccentricity.

Proof of the accuracy of this inference, by computation on the elliptic hypothesis of Ward.

By the elliptic hypotheſis, the analogy for converting mean into equated anomaly is ſimply this: *As the aphelion diſtance : is to the perihelion diſtance :: ſo is the tangent of half the mean anomaly : to the tangent of half the equated anomaly;* and the difference between theſe two anomalies conſtitutes the equation itſelf. Now, it is well known to all who are converſant in the theory of planetary motion, that in the projection of any elliptic orbit, a *circle,* deſcribed from the focus in which the ſun is ſuppoſed to be, with a radius that is a mean proportional between the major and minor ſemi-axes, will *cut the ellipſe* in *two points,* which ſhall be the points of *mean diſtance;* or, which is the ſame thing, the points where the equation becomes *ſtationary,* and conſequently where it is a *maximum.* It is alſo equally well known to practical aſtronomers, and calculators of an ephemeris, that the equation varies very ſlowly for many degrees both before and after the points of mean anomaly correſponding to the greateſt equation; and likewiſe that theſe points fall a little beyond the firſt quadrant from the aphelion, or three degrees of mean anomaly, by a quantity which depends upon the eccentricity of the orbit. In the orbit of Mercury the point of mean anomaly, when the equation is greateſt, is nearly at 105° from the aphelion; in that of Venus it is between 90° and 91°; in that of the Earth about 91°: in that of Mars about 97°; in that of Jupiter and Georgian between 93° and 94°; and in that of Saturn about 94°. Hence it may be inferred, that if the greateſt equation of Ceres be 3° 25′, the ſaid point of mean anomaly will be about 92°: but that if the equation be 9° 27′ 41″, it will be about 96°; namely, ſomewhat ſhort of that of Mars, the greateſt equation of which is 10° 40′ 40″.

Let us try now what the greateſt equation will be upon both ſuppoſitions ſucceſſively, according to the *ſimple elliptic hypotheſis.*

		Log.
As the aphelion diſtance	(27673+825) 28498	4,45481
Is to the perihelion diſt.	(27673—825) 26848	4,42891
So is the tangent of 46° ($\frac{92°}{2}$) ½ mean anom.		10,01516
		14,44407
		4,45481
To the tangent of ½ eq. anom. 44° 17′ nearly		9,98926

Then 92°—88° 34′=3° 26′ is the greateſt equation.

Again,

Again, ſuppoſing the point of mean diſtance to be at 96°, we have in that caſe,

As the aphelion diſtance, 28498 - - - - - -	4,45481
Is to the perihelion diſt. 26848 - - - - - -	4,42891
So is the tangent of 48° - - - - - - - - -	10,04556
	14,47447
	4,45481
To the tangent of 46° 17′ nearly - - - - -	10,01966

Then 96°—82° 34′=3° 26′ is the greateſt equation, as before.

Hence it is indubitably proved, that the equation, as given by Gauſs, is much too great for the eccentricity; and it appears alſo, according to what has been already aſſerted, that the equation at 92° and 96° of mean anomaly is *nearly the ſame*; that is to ſay, the difference will only be in the *ſeconds*. But the greateſt equation of a planet is uſually determined from a ſeries of obſervations antecedently to the calculation of the eccentricity: therefore the error which has been detected may be in the eccentricity, and not in the equation; in which caſe, by reverſing the analogy already uſed, we ſhall have this calculation, viz. As unity : mean diſtance 27673 :: natural ſine of 4° 43′ 50½″ or 08247,4 : 2282,2 for the requiſite eccentricity. But it will be moſt eaſy to determine in which of the two elements the error has been committed, when the whole period has been accurately aſcertained.

The equation given by Gauſs is too great for the eccentricity; or the eccentricity is faulty: but this will be beſt known from the period, when determined.

When it was mentioned in the former paper on this ſubject, that oppoſitions and conjunctions were of importance to be obſerved, the reaſon was omitted to be explained; which is, that when a ſuperior planet is in oppoſition, or an inferior one in conjunction, the obſerved geocentric longitudes are alſo heliocentric longitudes, without calculation or reference to diſtance and eccentricity; becauſe in ſuch relative ſituations there is no parallax of the orb: and it is well known to aſtronomers, that when an oppoſition happens at the place of mean diſtance of a ſuperior planet, *half the difference* between the heliocentric place, by calculation of mean motion, and of the place as obſerved at that time, is equal to the *greateſt equation*. The 13th of March ult. was the day on which the aſtronomers on the continent predicted that an oppoſition of Ceres would

Oſervations of conjunctions and oppoſitions, and their uſe.

 occur;

occur; but it muſt have happened on the 23d, as I calculate from Von Zach's little ephemeris continued forwards; viz. when the geocentric plane was about 182°. The aſtronomer who has an obſervatory, and has noted the *exact time*, will do well to make the obſervation public.

Application to the planet Ceres.

The mean time which elapſes between two ſucceſſive oppoſitions or conjunctions of a planet, as ſeen from the earth, is called a ſynodic revolution, and is determined by dividing 360° by the difference of the mean daily motions of the earth and other planets. Thus: taking the mean daily motion of Ceres at 770,7376″, according to Gauſs, and of the earth at 58′ 8,33″, according to Lalande, we have $\frac{360^\circ}{2777,5924} = 466,6$ days nearly for the whole ſynodic period, on a ſuppoſition that the motions are both equable throughout their orbits; but their reſpective diſtances from their aphelia at the time of oppoſition muſt be made the argument of a correction, either additive or ſubtractive, as the caſe may be, to determine what a ſynodic period would be if both motions were equable. Now, if we reverſe this proceſs, we can juſt as eaſily gain the difference of the daily motions between that of the earth and other planets, and conſequently the whole period of the latter, from having only the earth's daily motion, and *obſerved ſynodic period*; for 360°, divided by this period in days, gives the difference wanted at once, which, ſubtracted from the daily motion of the earth, gives that of the other, if it be a ſuperior planet; but if an inferior one, that difference muſt be added; and the more nearly the two daily motions approximate to each other, the longer will be the reſpective ſynodic revolution. In the inſtance before us, if we ſuppoſe the whole corrected ſynodic revolution of Ceres to be 466,6 days from obſervation, we ſhall have $\frac{360^\circ}{466,6} = 2777,5924''$ for the *difference* to be ſubtracted from 3548,33″ the earth's mean daily motion, which will leave 770,7376″ for the mean daily motion of Ceres, as before; by which if we divide 360°, we ſhall have the whole tropical period $= 1681^d\ 12^h\ 8^m\ 49^s$. But it remains to be obſerved what a whole ſynodic period of Ceres *may prove in reality*.

Determination of the real period, &c.

Suppoſing the epoch, or mean heliocentric longitude of Ceres to have been 2 S. 17° 36′ 34″ on January 1, 1801, the day of its diſcovery, as is ſtated by Gauſs, and the place of the aphelion 10 S. 26° 27′ 38″, the mean anomaly muſt, on

this ſuppoſition, have been at that time 3 S. 21° 8′ 56″, ſo that it had paſſed the place of mean motion about either 19° or 15°, accordingly as we make the greateſt equation 3° 25′ or 9° 27′ 41″: therefore the daily motion was nearer a mean motion than it has been ever ſince; and it will be yet ſome months before it arrives at its place of mean motion in the oppoſite half of its orbit; which place is either 2° or 6° ſhort of the *ninth* ſign of anomaly, accordingly as we take the eccentricity. Let us ſuppoſe now the whole period to be upwards of 1681 days, as has been, perhaps prematurely, determined; one fourth of this time had elapſed on the 24th of February laſt; on which ſuppoſition, the *mean anomaly* muſt then have been advanced juſt *three ſigns* from the original ſituation; namely, it muſt have been upwards of 6 S. 21°, at which rate the planet had paſſed the perihelion by a ſpace of time anſwering to 21° of mean motion, which is about 98 days: therefore the 18th of November, 1801, muſt have been the day on which it was at the perihelion, or place of greateſt velocity; but at that time the planet *was loſt*, and we are not in poſſeſſion of any obſervation of it nearer that time than the 7th of December following, when Baron Von Zach re-diſcovered it.

The continuance of any planet in the firſt quadrant from aphelion is longer than in the ſecond quadrant, by a ſpace of time which correſponds to the whole equation, taken at three ſigns of mean anomaly; in which ſituation, it has been already obſerved, that the equated or apparent motion is alſo, as nearly as may be, a mean motion; if therefore the equation at three ſigns be divided by the mean daily rate of motion, we ſhall have a ſpace of time, which, added to one fourth of the whole period, and ſubtracted from another fourth, will give nearly the reſpective times of continuance in the firſt and ſecond quadrants of anomaly: Hence ariſes this rule for finding the two ſemicircles, reſpectively biſected by the perihelion and aphelion points, viz. divide four times the equation at three ſigns of anomaly, (which may be the greateſt equation where the eccentricity is ſmall), by the mean daily motion, and the quotient will be the number of days that the planet continues longer in the ſemicircle from nine to three ſigns of anomaly than from three to nine. For inſtance, if we take the equation

tion of Ceres at 3° 25′, we ſhall have $\frac{3^\circ 25' \times 4}{77^\circ 7376''} = 63{,}83$ days for the time of continuance in the ſemicircle embracing the aphelion, longer than in the ſemicircle which is biſected by the perihelion: but if we take the equation at three ſigns = 9° 25′, ſomewhat leſs than the greateſt equation, in this caſe, by reaſon of the increaſed eccentricity, we ſhall have the exceſs of continuance $\frac{9^\circ 25' \times 4}{77^\circ 7376} = 175{,}93$ days.

Obſervations from which the poſition of the apſides are deduced, &c. &c.

This ſuggeſtion may be worthy the notice of the practical aſtronomer; for when a variety of obſervations are taken of the new planet in the different quadrants of its orbit, and the correſponding times recorded, it will be no difficult taſk, when equidiſtant geocentric longitudes are converted into heliocentric longitudes, to obſerve what ſemicircle of the ecliptic correſponds to that half of the orbit in which the planet has *continued longeſt*: the middle of that ſemicircle will be the *aphelion*, and the two extremities will be three and nine ſigns of anomaly: Alſo, the *exceſs of duration*, above the time occupied by the other ſemicircle, multiplied by the *mean daily motion*, will be *four times* the equation at three and nine ſigns of anomaly very nearly; and as this equation is very little ſhort of the greateſt equation, the eccentricity may likewiſe be found by either of the methods already deſcribed: Thus the form and elementary points of the orbit may be gained by a ſeries of obſervations converted into heliocentric places, even by the projection propoſed in the laſt memoir on this ſubject, and theſe determinations may be corrected by a compariſon of them with the reſults deduced from the properties of an ellipſe, which are here purpoſely omitted, leſt a more minute and ſcientific deſcription of intricate calculations ſhould rather puzzle than inform the generality of readers *.

Inclination of the orbit, and place of nodes.

It remains yet that ſome obſervations be made relative to the *poſition* of the orbit of a planet. There are many methods of aſcertaining the nodes of a planet's orbit, from calculation grounded upon obſervations; but the ſimpleſt, when it is practicable, is to convert the geocentric into the heliocentric place at the time when there is no latitude by obſervation, for

* See Lalande's and Mr. Vince's Aſtronomy; and alſo Profeſſor Robiſon on the Geo. Sidus, in the Edin. Tranſ. vol. I. 1788.

the

the heliocentric place will be the place of the node, afcending or defcending, as the cafe may be, which will appear by a fubfequent obfervation; but when the place of a planet, when croffing the ecliptic, cannot be obferved, the *middle point* between two *equal north* and *fouth* latitudes, gained by obfervation, will give the node.

The heliocentric latitude, when a planet is juft 90° from each node, is the meafure of the inclination of its orbit, and is eafily obtained from the obferved geocentric latitude, taken in that fituation, by the analogy already defcribed; or, otherwife, the greateft heliocentric latitude may be acquired from an obfervation of a geocentric latitude and longitudinal diftance from the node, thus: When the earth is in the line of the nodes, the analogy will be, *as the fine of the difference of the longitudes of the fun and planet feen from the earth* : *radius* :: *tangent of the geocentric latitude* : *tangent of the inclination.*

Days when the earth will be in the line of the nodes of Ceres.

The two days on which the earth will be in the line of the nodes of Ceres will be June 12. and December 13. this year. But it is beyond the propofed intention of this popular memoir to enter into all the minutiæ of calculation, were the requifite data before me; but only to point out the *methods* of applying obfervations for determining the fize, form, and pofition of a planet's orbit: it may not, however, be unworthy of notice, before I conclude, to remark, that the aftronomers on the continent, who availed themfelves of the earlieft obfervations *only* for determining an approximate fet of elements of Ceres, were enabled to do this from noticing that this planet became *ftationary* between the 10th and 11th of January, 1801, when its elongation was known by obfervations; for it has been fhewn by writers on aftronomy, that, upon a fuppofition of circular orbits, *the tangent of the elongation is equal to the femidiameter of the orbit, divided by the fquare root of that femidiameter* + 1.

The early determinations refpecting the orbit of Ceres were made from its ftationary pofition.

Your's, &c.

W. P.

APPENDIX.

APPENDIX.

April 10, 1802.

Since the preceding paper on the planet Ceres was written, Mr. Ed. Troughton has put into my hands the duplicate of another letter, ſent to him by the Baron de Zach, and addreſſed to Sir Joſeph Banks, Bart. which I underſtand has been read at a meeting of the Royal Society, and which I ſhall here tranſcribe *, on account of ſome remarks which I have to make upon it. (Copy,)

Seeberg Obſervatory near Gotha, Feb. 20, 1802.

" DEAR SIR,

Letter from Baron von Zach.

" I had the honour to ſend to you my obſervations of the new planet Ceres Ferdinandea made in January, here I take the liberty to ſend the continuation of them made in February.

Table of obſervations.

1802.	Mean Time in Seeberg.			Ap. Right Aſcen. obſerved.			App. Dec. obſerved.		
Feb. 3	15^h	40′	35″ S.	188°	42′	13,05″	12°	40′	5″ N.
4	15	36	41,4	188	42	36,30			
5	15	32	45,1	188	42	30,15	12	50	25
9	15	16	43,7	188	38	3,90	13	14	18
19	14	34	46,7	187	58	27,90	14	20	3

Dr. Gauſs has corrected his elliptical elements of the orbit upon my obſervations; here is what he has found ſince my laſt letter to you.

Elements.

Epoch for the beginning of the year to the meridian of Seeberg - - - - 77° 27′ 36,5″

Aphelion } both ſidereal, - - 325 57 15,0
Node } - - - 80 58 40,0

Greateſt equation of the center - - 9 20 8,0

Inclination of the orbit - - - 10 37 56,6

Logarithm of $\frac{1}{2}$ axis major 0,4424742

Eccentricity of the orbit 0,0814064

Mean diurnal heliocentric and tropical motion 769″,7924

With theſe elements of the orbit all the obſervations made by Mr. Piazzi in Palermo, from Jan. 1, till Feb. 11, 1801,

* The letter was ſent open to Mr. Troughton for the expreſs purpoſe of copying and communicating the ſame. W. N.

agree

agree perfectly well, and within a few seconds; and my observations are represented by them thus:

Seeberg observ.	R. A. calculated.	Differ.	Declin. calcul.	Differ.
1801, Dec. 7	178° 33′ 29,2″	— 1,4″		
1802, Jan. 11	186 45 47,6	— 2,3		
16	187 27 38,8	—14,4		
22	188 6 18,2	— 7,6	° ′ ″	
25	188 20 37,2	— 2,0	11 56 58,4	+35,4″
26	188 24 37,0	—12,5		
28	188 31 25,7	—12,1	12 9 55,6	+14,3
29	188 34 14,1	— 4,0		
30	188 36 38,4	— 5,5	12 19 19,8	+19,1
31	188 38 38,3	— 7,1	12 24 15,3	
Feb. 3	188 42 7,8	— 5,2	12 39 53,6	—11,4

As these elements agree hitherto so well with the heavens, the following ephemeris calculated upon them for the next month, will probablydo the same, so I annex it here to point out to the English observers the place where they have to look for the Ceres.

Position of the Ceres for Midnight Mean Time in Seeberg Observatory. Ephemeris.

1802.	R. A. in degrees.	Decl. N.	R. A. in Time.
March 1	186° 41′	15° 30′	12h 26′ 45″
4	186 11	15 50	12 24 45
7	185 39	16 10	12 22 36
10	185 5	16 29	12 20 18
13	184 28	16 47	12 17 53
16	183 51	17 4	12 15 24
19	183 13	17 19	12 12 50
22	182 34	17 33	12 10 15
25	181 55	17 44	12 7 40
28	181 17	17 54	12 5 7
31	189 39	18 1	12 2 37
April 3	180 3	18 6	12 0 12
6	178 29	18 10	11 57 54

This planet will come in opposition to the sun, March 17 in the afternoon. At the same time this heavenly body will be in its greatest proximity to the earth = 1,6025, and therefore

fore the moſt favourable time to look for its ſatellites, if there are any, to meaſure its diameter; and to examine its nebuloſity. About this time, the planet will alſo be in its greateſt geocentrical latitude = 17° 9′, and a little later ſhe will have her greateſt retrograde motion, about 13 min. in right aſcenſion per day. The north declination will increaſe till the beginning of April, and about the 9th of the ſame month the motion in declination will commence to the ſouth. It appeared to me that the Ceres has ſome change of light; I imputed it at firſt to our hazy atmoſphere this winter, but Mr. Schroeter of Lilienthal, and Mr. Olbers of Bremen, ſent me word that they have obſerved the ſame, and they believe that it is the planet which is ſubject to ſuch changes of light. Mr. Herſchel will tell us beſt whether it is ſo. I have ſome hopes to find the planet in ancient catalogues of ſtars. Mr. Meſſier was very near it in the year 1779. The famous comet of that year ran juſt over the northern wing of Virgo, as now, and the new planet was not very far diſtant. If the comet had attained two months ſooner the completion of Virgo, Mr. Meſſier muſt infallibly have obſerved the Ceres then, becauſe he determined all the little ſtars in the vicinity of the comet; the planet would have been in the way of the comet, and ſo of courſe he would have catched the little planet in 1799.

Variable light of Ceres.

If my obſervations are acceptable to you, dear Sir, only a little hint, and I ſhall continue with pleaſure to give you further intelligence.

I am,
with the greateſt eſteem and regard,
very reſpectfully,
moſt honoured SIR,
Your obedient humble Servant,
FRANCIS BARON DE ZACH.
Lieut. Col. and Director of Seeberg Obſervatory near Gotha Saxony."

REMARKS.

Remarks.

1. The diſtance which correſponds to the logarithm of $\frac{1}{2}$ axis major, viz. 0,4424742, is 2,769964, the earth's radius being *unity*.

2. The whole tropical period from the mean daily heliocentric tropical motion, 769,7924″, is 1683^{d} 13^{h} 41′ 56,3″.

3. The

3. The ſynodic revolution correſponding to this motion is 466^d 10^h 22^m.

4. The time of oppoſition could not be on the 17th of March as ſtated in this letter, but about the 23d, as has been mentioned before, becauſe it was on that day that the difference of the right aſcenſions of the ſun and Ceres was 180°, even according to the Baron's own table: the error ſeems to have ariſen from reckoning the point diametrically oppoſite Ceres to be nearly two degrees *ſhort of* the equinoctial point, inſtead of the ſame quantity *over*, when the right aſcenſion of Ceres was about 182°: the other circumſtances alſo dependent on the moment of oppoſition muſt therefore be attributed to the 23d inſtead of the 17th.

Cauſe of the apparent diſagreement of ſome deductions of the author, and the elements of Mr. Gauſs.

5. On receiving theſe laſt corrections of Dr. Gauſs I was at firſt ſurpriſed to find ſuch a trifling alteration made with the greateſt equation and correſponding eccentricity, after the error which I was confident I had detected; but I have now found out the cauſe of the apparent diſcrepancy, which ſome ſtreſs has been laid upon; the mean diſtance and eccentricity, I now perceive are, contrary to the uſual mode of expreſſion, given in terms of *different denominations:* the mean *diſtance* has been given in terms which ſuppoſe the radius of the *earth's orbit to be unity,* and the *eccentricity* is given in terms which ſuppoſe the radius of the *orbit of Ceres to be unity,* inſtead of its *proportional* radius 2,769964. Profeſſor Robiſon on the contrary, in his approximate elements of Georgian expreſſed the mean diſtance and eccentricity in terms of the ſame denomination, which is alſo done by Lalande, Vince, and other eminent aſtronomers with reſpect to the other planets. Let us try now what the greateſt equation will be by the elliptic hypotheſis, when *unity* is made the radius of the orbit:

As the aphelion diſtance (1+,08140)	1,08140—	4,03383
Is to perihelion diſtance (1—,08140)	9186—	3,96313
So is tang: of 46° $\frac{1}{2}$ mean anom:	-	10,01556
		4,00869
To tang: $\frac{1}{2}$ eq. anom: 43° 20′ 33,6″	-	4,03383
		9,97486

Then 48° 20′ 33,5″ + 2 = 86° 41′ 72″; and 92° — 86° 41′ 7,2″ = 9° 18′ 5,28″ is the greateſt equation.

 Alſo

Alſo by the tabulated method we have $\frac{8140}{100000} = 08140$, which is the natural ſine of 4° 40′ 8,27″, or half the greateſt equation 9° 20′ 16,51″, which is not 9″ above the correction of Dr. Gauſs.

6. Hence it appears, that the eccentric point in the projection of the orbit of Ceres ſhould be a little leſs than $\frac{1}{12}$ of the radius from the central point S (Plate XIII. Fig. 1.) which repreſents the ſun.

W. P.

XI.

Deſcription of a very cheap Engine for raiſing Water. In a Letter from Mr. H. SARJEANT *of Whitehaven, to* Mr. TAYLOR, *Secretary to the Society for the Encouragement of Arts.**

SIR,

Introduction.

I AM ſenſible that the little engine, a drawing of which accompanies this letter, can lay no great claim to novelty in its principle; nevertheleſs it is reſpectfully ſubmitted to the conſideration of the ſociety, how far its ſimplicity, and cheapneſs of conſtruction, may render it worthy of their attention, with a view to its being more generally known and uſed in ſimilar caſes.

Height of Irton-Hall 60 feet above the ſtream.

Irton-Hall, the ſeat of E. L. Irton, Eſq. is ſituated on an aſcent of ſixty or ſixty-one feet perpendicular height; at the foot of which, at the diſtance of about 140 yards from the offices, runs a ſmall ſtream of water. The object was to raiſe this to the houſe for domeſtic purpoſes.

To this end a dam was made at a ſhort diſtance above, ſo as to cauſe a fall of about four feet; and the water was brought by a wooden trough, into which was inſerted a piece of two-inch leaden pipe, a part of which is ſeen at A, plate 2.

Deſcription of the engine. A bucket is ſuſpended at one end of a beam and a counter-weight at the other end. The ſtream fills the bucket and raiſes the counter-weight;

The ſtream of this pipe is ſo directed as to run into the bucket B, when the bucket is elevated; but ſo ſoon as it begins to deſcend, the ſtream flows over it, and goes to ſupply the wooden trough or well in which the foot of the forcing pump C ſtands, of three inches bore.

D, is an iron cylinder attached to the pump rod, which paſſes through it. It is filled with lead, and weighs about 210 lbs.

* From the Tranſactions of the Society, for 1801, page 255. The ſilver medal was given to the Inventor.

240 lbs. This is the power which works the pump, and forces the water through 420 feet of inch pipe from the pump up to the houfe.

and this laft in its defcent works a fmall force-pump.

At E is fixed a cord which, when the bucket comes to within four or five inches of its loweft projection, becomes ftretched and opens a valve in the bottom of it, through which the water empties itfelf.

The bucket is made to empty by a valve and ftring.

I beg leave to add, that an engine, in a great degree fimilar to this, was erected fome years ago by the late James Spedding, Efq. for a lead-mine near Kefwick, with the addition of a fmaller bucket which emptied itfelf into the larger, near the beginning of its defcent, without which addition it was found that the beam only acquired a libratory motion, without making a full and effective ftroke.

To anfwer this purpofe in a more fimple way, I conftructed the fmall engine in fuch manner as to finifh its ftroke (fpeaking of the bucket end,) when the beam comes into an horizontal pofition, or a little below it. By this means the lever is virtually lengthened in its defcent in the proportion of the radius to the cofine, of about thirty degrees, or as feven to fix nearly, and confequently its power is increafed in an equal proportion.

Contrivance to complete the ftroke.

It is evident that the opening of the valve might have been effected, perhaps better, by a projecting pin at the bottom; but I chofe to give an exact defcription of the engine as it ftands. It has now been fix months in ufe, and completely anfwers the purpofe intended.

The only artifts employed, except the plumber, were a country blackfmith and carpenter; and the whole coft, exclufive of the pump and pipes, did not amount to £5.

It is very cheap.

I am, Sir,

Your humble fervant,

H. SARJEANT.

Warwick Court, Holborn.

Mr. Charles Taylor.

In another letter, dated Whitehaven, April 28, 1801, Mr. Sarjeant further obferves that the pump requires about eighteen gallons of water in the bucket to raife the counter-weight, and make a frefh ftroke in the pump; that it makes three ftrokes in a minute, and gives about a half-gallon into the ciftern

With a fall of 4 feet and confumption of 18 gallons, it raifes half a gallon thro' 60 feet. That is to fay 12 parts of

water raiſe 5 parts. Its rate is about one eighth part of one man's work; and it throws up 24 hogſheads in the day.

tern at each ſtroke. He adds, " I ſpeak of what it did in the dryeſt part of laſt ſummer; when it ſupplied a large family, together with work-people, &c. with water for all purpoſes, in a ſituation where none was to be had before, except ſome bad water from a common pump which has been ſince removed. But the above ſupply being more than ſufficient, the machine is occaſionally ſtopped to prevent wear, which is done by merely caſting off the ſtring of the bucket valve."

XII.

Concerning the Identity of Tellurium and Antimony, the galvanic Effects of Magnetiſm, and other Philoſophical Subjects. By a CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

Hiſtory of the intelligence.

ANXIOUS to learn the particulars as well as the truth of the intelligence which I received from Moravia concerning the *Tellurium* and the *decompoſition of water* pretended to have been effected at Vienna *by means of the magnetic fluid*,* I applied to a chemical friend reſiding there; and it appears that this intelligence was not correct. Whence to prevent farther miſinformation I conceive it my duty to haſten in publiſhing the following extract from my correſpondent's anſwer to my letter; and to requeſt you the favour of inſerting it in your excellent Journal, if there be room for it.

I have the honour to be,

Sir,

Your humble ſervant,

N. N.

London, April 21. 1802.

Extract of a Letter from Vienna, dated 30th March, 1802.

" The intelligence communicated to you from Moravia concerning the Tellurium and decompoſition of water was a little premature, and is not farther true than as follows:"

Account of the experiments that render it probable that Tellurium is antimony.

" As to the firſt point Mr. *Tcharſky,* major in the artillery, (well known by his refutation of Tondi's experiments† towards

* See *Philoſophical Journal,* new Series, vol. I. pag. 234.

† An account of this is given in Baron *Born*'s ſyſtematic catalogue of the collection of foſſils of Mlle de Raab at Vienna. Tranſl.

wards eſtabliſhing the poſſibility of reducing barytes, lime and magneſia to metallic reguli,) had reſolved to employ his conſiderable ſtock of *Tellurite* and *Uranite* for the purpoſe of ſubjecting theſe two metals to a new and cloſer examination. He, therefore, firſt prepared, according to Klaproth's manner ‡ a regulus of Tellurium, weighing ſeveral grains. This regulus perfectly agrees with reguline antimony, *only* with reſpect to its *phyſical properties*, for inſtance, in the fracture, colour, hardneſs, ſpecific gravity; and ſo likewiſe its oxyde perfectly reſembles the *antimonium diaphoreticum ablutum* (lixiviated white oxyde of antimony by nitre.) However, it would be premature, without farther experiments, in which Major Tſcharſky is at preſent engaged, directly to conclude that Tellurium is nothing elſe but reguline antimony."

"Major Tſcharſky has alſo prepared many ounces of the oxyde of Uranium, and will in a ſhort time reduce it to a regulus of conſiderable ſize for the purpoſe of farther experiments." Uranium.

"Concerning the ſecond point it relates only to an experiment of Profeſſor *Jordan*, who, as in the galvanic experiment for decompoſing water, connected metallic wires immerſed in water with the oppoſite poles of a magnet, and obſerved that an oxydation of the wires was effected. But as this experiment would not ſucceed in the ſeveral inſtances in which it was repeated, Major Tſcharſky and Captain *Lethenyey* are at preſent occupied in preparing a large magnetic apparatus in order to inſtitute a regular ſeries of experiments on this ſubject." Suppoſed galvanic effects of the magnet.

J.

Poſtcript. Chevalier *Landriani* affirms that he has diſcovered a method of copying old writings in the ſame manner, as of recently written papers, duplicates are taken by means of copying-machines.—He has alſo prepared an excellent indelible ink, the compoſition of which he will ſoon publiſh. Landriani's method of copying old MSS. Indelible ink.

‡ The proceſs by which Klaproth eſtabliſhed the Tellurium as a new metal has been given in *Nicholſon*'s Journal, vol. II. 4to. 1799, page 273. Tranſl.

SCIENTIFIC

SCIENTIFIC NEWS, &c.

Notice respecting the Discovery and Situation of chromated Iron in France.

Chromate of iron.

C. Pontier has already found, three years ago, in the lower Alps, some fragments of chromated iron, out of its place; but circumstances and the war prevented him from discovering the true position of this new and curious mineral in the earth. He, however, at last found it in its natural place in a quarry near Gassin, in the road to Cavalaire.

This metal is mixed with a green serpentine rock, which probably owes its colour to the chrome, according to C. Pontier's opinion. It sometimes forms masses of five solid decimeters each.—*Bulletin des Sc. No* 57.

BOOKS OF SCIENCE,

A Treatise on Astronomy, in which the Elements of the Science are deduced in a natural order from the Appearances of the Heavens to an Observer on the Earth; demonstrated on Mathematical Principles; and explained by an Application to the various Phenomena. By OLINTHUS GREGORY, *Teacher of Mathematics, Cambridge. Octavo, 522, very full Pages, with nine Copper Plates, exclusive of the Preface, Table of Contents and Index,* Kearsley, London.

IT must be admitted that we have few works in our language in which the extensive science of astronomy is fully treated; and though we have several excellent popular compendiums, yet a work was wanted in which a familiar deduction of the effects from the phenomena should be combined with the stricter principles of mathematics, and in which a large quantity of useful and interesting matter should be given in the compass of an octavo volume. These are the leading advantages of the present work, besides that of its recent appearance, which naturally enabled the author to enrich his treatise with the interesting discoveries of later times.

His order consists in an investigation of the figure and dimensions of the earth; the use and meaning of the imaginary circles and points; apparent motions; causes of seasons, &c. the fixed stars; parallax, refraction, equation of time; systems of astronomy; truth of the Copernican system; theory of apparent motions; law of the planetary revolutions; real motions, rotations; phenomena of secondaries; eclipses, transits, cometary phenomena; aberration of light; elements of computation, tables, &c. &c. L. L.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE, 1802.

ARTICLE I.

On the Rev. Mr. Pearſon's Analogy for deducing the greateſt Equation from the Eccentricity. In a Letter from Mr. OLINTHUS GREGORY.

Cambridge, May 7, 1802.

To Mr. NICHOLSON.

SIR,

THE brilliant diſcoveries which have been lately made by aſtronomers in different parts of Europe, have naturally produced a ſpirit of inquiry in many perſons who have hitherto paid little regard to ſcientific ſubjects, and, at the ſame time, have excited an earneſt and active ſolicitude in thoſe who have made conſiderable advances in the cauſe of ſcience, that theſe inquirers be rightly directed in their purſuit. Among thoſe who have thus laudably exerted themſelves in the promotion of uſeful knowledge, your ingenious correſpondent the Rev. Mr. Pearſon, of Lincoln, (now of Parſon's Green) muſt certainly be enumerated; and his many able communications to your Journal demand the thanks of its numerous readers. In the laſt of that gentleman's valuable papers on the planet *Ceres Ferdinandia,*

Intereſting diſcoveries in aſtronomy.

Mr. Pearſon's method of deducing the greateſt equation.

Ferdinandia, he mentions a ſimple method of deducing the greateſt equation of a planet's centre from the excentricity, which he diſcovered a few years ago; and he invites your mathematical correſpondents to give a demonſtration of this method, reſpecting the accuracy of which he ſeems to have little doubt. In conſequence of Mr. Pearſon's invitation, I take the liberty of addreſſing you on the ſubject. It will ſave much circumlocution to refer to a diagram: let, therefore, the ellipſis A P G Q in the annexed figure 1. Pl. VII. repreſent the orbit of a planet moving about the ſun S in one of the foci; and let A N Q E be a circle deſcribed upon the major axis of the orbit as a diameter. Then, if P be the place of the planet in its orbit, A N will meaſure the excentric anomaly; alſo, if the arc A D be taken proportional to the time from the aphelion, it will repreſent the mean anomaly; and, letting fall the perpendicular S T from S upon N C continued, it is ſhewn, by *Keill* and many others, that $AN + ST = AD$. It has alſo been ſhewn that, when the equation of the centre is a maximum in any orbit, the diſtance S P of the planet from the ſun is $= \sqrt{SA \times SQ}$, and when this is the caſe, in all orbits of ſmall eccentricity the point P nearly coincides with G, or the place of the planet will be near an extremity of its orbit's minor axis. In this ſituation of S P, it is manifeſt that S T will be almoſt coincident with S C, and nearly equal to it; S D and C N will be nearly parallel; and $DCN + SNC$ nearly equal to $2\,DCN$; that is, the greateſt equation will be nearly equal to $2\,ND$, or $2\,ST$, or $2\,SC$. But, when G S the mean diſtance repreſents the radius of a circle, as the angle G C S is 90°, S C will manifeſtly repreſent the *ſine* of an angle: therefore, as G S, the mean diſtance, to S C, the eccentricity, ſo is 1, or radius, to the ſine of $\frac{1}{2}$ the greateſt equation, which is the ſame as Mr. Pearſon's analogy.

Demonſtration of its ground and accuracy.

It is merely an approximation.

It is obvious from this inveſtigation, that the ſimple method pointed out by Mr. P. is merely an approximation. It ſuppoſes that the orbits are nearly circular, and will therefore produce a reſult deviating moſt widely from the truth when the eccentricity is the greateſt. Thus in Mr. P's table (p. 49, No. V. N. S.) the greateſt error is in the orbit of mercury, where the eccentricity is ·20551 in terms of its own mean diſtance. The next greateſt error is in the orbit of Mars, where the eccentricity is ·09308. The next error in order is that

that of Ceres, where the eccentricity is ·08140, and error $4\frac{1}{2}''$. (Pa. 60. No. V.) In the caſe of the Georgian the error or difference, as ſtated in the table, is much too great to correſpond with the eccentricity. But here I conceive there is a miſtake in the determination of the eccentricity: for, according to a mean from various aſtronomers (*Mechain, Hennert, De la Place, Zach,* and *Robiſon,*) the proportional mean diſtance of the Georgian is not 1918352, but 1908352. Hence then, we have $\frac{90804}{1908352} =$ ·0475824 = nat. ſine of $2^\circ\ 43'\ 38{\cdot}4''$, differing $\frac{4}{10}$ of a ſecond from the determination of *Lalande.* And, in the caſe of Jupiter, where the eccentricity is ·04807, the correſponding difference is $\frac{55}{100}$ of a ſecond; which is a little larger than the former, as it ought to be. So that it appears, as well from Mr. P's table as from the inveſtigation, that this method does not furniſh a reſult ſufficiently accurate except when the eccentricity is pretty ſmall.

The converſe analogy to Mr. Pearſon's long known.

Before I conclude, I would juſt beg to remark, that this method, though new to Mr. Pearſon, is in reality well known. An analogy which is, in fact, the converſe of this, though expreſſed rather leſs commodiouſly, is given with a demonſtration, in La Caille's Aſtronomy, art. 147. Vince's Aſtron. art. 231. and my Aſtron. art. 344. And I believe it is alſo given in the excellent work of La Lande; but as I have not that performance at hand, I cannot now refer to the place. Another analogy preciſely to the ſame effect, and deduced from the ſame principles, may be ſeen in art. 249. Book V. Robertſon's Navigation, and under the article ECCENTRICITY in Dr. Hutton's Math. and Phil. Dictionary. It muſt afford great ſatisfaction to a gentleman of Mr. Pearſon's candour and liberality, to find that the analogy which he accidentally ſtruck out, is the converſe of one which admits of a ſimilar demonſtration, and has been long uſed by aſtronomers as an eaſy and excellent approximation.

I am, Sir,

Your's with much reſpect,

OLINTHUS GREGORY.

II. *On*

II.

On the Conſtruction of the Beams of Steam Engines. By Mr. J. C. HORNBLOWER. *From the Author.*

DEAR SIR,

Hiſtorical introduction.

I BEG leave through the means of your Journal, to lay before the public an account of the framed lever mentioned at the cloſe of the article *Carpentry* in the ſupplement to the Encyclopedia Britannica, as it was originally deſigned for an engine to have been erected at Amſterdam in the year 1776, together with two others, poſſeſſing every poſſible advantage of levers conſiſting of ſmall ſcantles.

The framed lever here deſcribed needs no hole bored in it. Other particulars.

I know not by what means the lever above referred to come to be conſtructed with the diſadvantages intimated by the writer of that article, but there is no neceſſity for a hole to be bored, or a bolt to be driven in any part of the framing between the arches, except for the chain ſtays. The wedges *a, b,* Fig. 1, Plate V. thus applied, would be an improvement, for want of which a lever of this ſort in the hands of a negligent engineman had one of its joggles forced off, the ſhoulder of the tennon, which was morticed into the arch, not being a joint by the eighth of an inch, or more; but when it met with the arch it went no further, and continued to work for many years under a great load, and much to its diſadvantage in other reſpects.

Dimenſions of a lever of indifferent workmanſhip.

The length of this lever was 21 feet, the ſcantles were 12 inches by 6; height of the whole when put together 30 inches, and leverage on the gudgeon as 4 to 3. This laſt circumſtance operated much againſt its conſtruction, by giving additional force againſt the joggle at that end, but had it been framed ſix inches higher, I doubt not but it would have ſtood to this day under all its diſadvantages.

Scantles and load.

The ſum of the ſcantles is 18 by 12, area of the ſection 216 inches, column of water in four lifts 4800 lb. with 440 fathoms of rods, (pump rods) which with the appendages on the other end, added to the power neceſſary to overcome the reſiſtance, amount to about ſeven tons.

Another conſtruction Fig. 2, well made.

But a much ſimpler, and in ſome reſpects a more advantageous mode of framing is ſhewn at Fig. 2, and may be conſtructed

ſtructed with or without arches. This, with little variation, is the invention of a Dutch gentleman, and was applied to the load of a 52 inch cylinder (an atmoſpherical engine) ſet up with advantages, which in point of workmanſhip at that time was perhaps not equalled, and therefore may be ſaid to have been fairly tried.

This engine was calculated to raiſe 60,000 gallons per minute, and the ſcantles were 18 by 12, and 12 by 8, and where ſuch timber can be had, it is hardly to be expected to have a lever with greater advantages than this for a ſingle ſtroke, and where a double ſtroke is required, it may be doubled for that purpoſe, retaining all its principles and properties as in Fig. 3. which I ſuppoſe needs no explanation. **Dimenſions and load.** **Double conſtruction.**

Fig. 4. is a lever conſtructed by an eminent engineer in Hungary ſome years ſince, which poſſeſſes a very great degree of ſupport by the king poſt and iron braces, but does not, in my opinion, diſcover ſo much ſcience as the two preceding ones. I forgot to obſerve, that inner arches may be attached to Fig. 1. without materially affecting its principle, if they are well let on the whole framing, and bolted to each other without paſſing through the ſcantles. **Strong framed lever in Hungary.**

I am, SIR,

Your moſt obedient Servant,

J. C. HORNBLOWER.

Eaſt Row, City Road, Tueſday, May 11, 1802.

III.

On the Theory of Chemiſtry. In a Letter from the Rev. J. PRIESTLEY, *L. L. D. F. R. S. &c,*

To Wm. NICHOLSON, ESQ.

DEAR SIR,

IN October laſt I ſent you a reply to Mr. Cruickſhank's obſervations on one of my arguments in ſupport of the doctrine of phlogiſton, in which I think I clearly ſhewed that he ſuppoſed fixed air to be formed in circumſtances in which it is impoſſible that it ſhould be formed, and that it is decompoſed by a ſubſtance which has no ſuch power. Having juſt received **Reference to former letter.** Vol. I. p. 181.

ceived a letter from a friend in Paris, in which I find that great account is made of the obſervations of Mr. Cruickſhank, ſo that it is now taken for granted that I muſt accede to the new theory, I beg you would add to my former letter, that Mr. Cruickſhank himſelf abandons the moſt fundamental principle of that theory, which is, that the only ſource of inflammable air of any kind is water; and he makes it not neceſſary for that purpoſe. Mr. Lavoiſier, treating of the inflammable air from charcoal and water, which is ſimilar to that from charcoal and finery cinder, ſays, *(Elements of Chymiſtry, p.* 87 *of the Engliſh tranſlation)* "It cannot poſſibly be diſengaged from the charcoal, and muſt conſequently be produced from the water." According to the new theory, the union of *oxigen,* which is ſuppoſed to come from the finery cinder, with carbon from the charcoal, muſt form *fixed air,* and not any kind that is inflammable. Mr. Cruickſhank therefore muſt abandon the new theory, in order to maintain his peculiar hypotheſis.

Mr. Cruickſhank abandons water as the ſole ſource of inflammable air. which Lavoiſier thought eſſential to his theory.

Inference.

If I do not receive a better defence of this new theory from its able ſupporters in France, I ſhall conclude it to be incapable of defence, and that, as becomes ingenuous men, they will abandon it, as Mr. Cruickſhank has virtually done.

I am, dear Sir,

Your's ſincerely,

Northumberland, Feb. 20, 1802. J. PRIESTLEY.

P. S. I have not any Number of your Journal of a later date than that for April laſt.

IV.

Experiments upon the tanning Principle, and Reflections upon the Art of Tanning. By CIT. MERAT GUILLOT, *Apothecary at Auxerre.*

Prouſt's proceſs for obtaining tanin tedious.

THE tediouſneſs of the proceſs indicated by Mr. Prouſt for obtaining the tanin, induced me to make ſome experiments, and to endeavour to find a more ſpeedy method of procuring it: the following is the reſult of my inquiries:

1. I

1. I infufed tan, in the ftate of fine powder, for fome hours in water; I filtrated this folution, and treated it with lime water; I obtained a precipitate in confiderable abundance, which I collected upon a filtre, dried, and afterwards treated with alcohol, in order to afcertain whether it were foluble in this menftruum; but the alcohol was not even coloured by it. Infufion of tan affords a precipitate by lime water.

2. Wifhing to afcertain whether the lime had a greater affinity for the acids than for the tannin with which it was combined, I treated four drachms of the precipitate of which I have fpoken above, with nitric acid diluted with water, with the acid of a very gentle heat; a pretty brifk effervefcence took place with a difengagement of carbonic acid gas; after four hours infufion, I filtrated the liquor, which had affumed a very deep tinge, and there remained upon the filtre a black pulverulent fubftance, brilliant, having an acerb and very flightly bitter tafte; this refiduum weighed a little lefs than two grammes. Acids difengage a pulverulent matter from the lime;

3. In order to afcertain whether the nitric acid had diffolved any lime, I treated the liquid which I had filtrated with the acidulous oxalate of potafh, and I obtained an abundant precipitate; on which account I conjectured, that fince the nitric acid had diffolved the lime, the fubftance which I had obtained upon the filtre muft be tanin, as the precipitate obtained by the mixture of the lime-water and of the infufion of tan, was produced by the union of the tanning principle and of the lime. In order to afcertain this point, I treated one portion of it with water, and the other with alcohol; I let thefe fubftances infufe in the fand bath for twenty-four hours: the water became ftrongly coloured, and the alcohol more fo; but all the tannin (hitherto I only prefume that it is fuch) was not diffolved; the alcohol diffolved only a little more than half of it, and the water lefs. I treated thefe two liquids, after having filtrated them, with a folution of glue, and I obtained a precipitate fimilar to that which is obtained by mixing infufion of tan with the fame folution, but of a much darker colour, and a little lefs elaftic. When I treated them with the muriate of tin, I obtained a precipitate which became gelatinous; when I treated them with lime-water, the tanin combined with the lime, and reproduced the tanate of lime already formed. by combining with the lime, and the pulverulent matter is tanin,

According

According to thefe properties, could I doubt that this was pure tanin? certainly not, fince it prefents the fame refults as that obtained by the procefs of Mr. Prouft.

How obtained pure.

If we wifh to obtain more pure tanin than that which is obtained after the folution of the lime of the tanate of this fubftance by an acid, the infufion in alcohol may be evaporated, and we fhall then have very pure tanin.

The muriatic acid has prefented me with the fame refult as the nitric.

Conjectures that lime water is advantageous in taning by the lime combining with the tan.

The rapidity with which the upper leathers of fhoes (*cuirs d'empeigne*) are tanned, according to the procefs of Citizen Lequin, who, in manufacturing them, contents himfelf with merely fubjecting them to the preparations of wafhing and flefhing by lime water, without fuffering them to fwell, and afterwards tans them, led me to prefume that in this cafe a combination is effected of tanin with the lime contained in the fkin thus treated, befides the combination of the tannin with the gelatine contained in the fkin, which accelerates this fabrication. May it not be probable according to this notion, that the fabrication of leather would be accelerated, if after having fubjected the fkins to the operations of wafhing and flefhing in lime water, they were left to fwell in the fpent ooze or water in which the old bark, which has already ferved for tanning leather, has been infufed. In this cafe, the fmall quantity of tanin diffolved in this water would combine with the lime with which the fkin would be charged in proportion to the working, and would form a tanate of lime. The fwelling would perhaps be effected by this means with a little lefs celerity than by the fulphuric acid, but then it would perhaps be preferable, from the circumftance that the fkin in fwelling would begin to charge itfelf with tanin, whereas by the fulphuric acid, the lime with which the fkin is impregnated, when worked,—this fubftance diffolved in water, the lime, I fay, combines with the fulphuric acid employed to fwell it, which, I prefume, muft give to the leather a brittle quality that it would perhaps not have if the other procefs were ufed. Perhaps alfo, after the fkins have fwelled in the ooze, this completion might be haftened by putting them firft into the folution of tan, as Cit. Lequin does, and afterwards fteeping them alternately in lime water and in infufion of tan, always taking care to leave them but a fhort time in the lime-water,

which

which might alter them if they were left in it too long. In this cafe, the lime-water with which the leather would charge itfelf, would determine a more fpeedy precipitation of the tanin, and its union with the lime as well as with the gelatine contained in the leather. I believe alfo that by this means the leather would acquire more weight, a quality in requeft amongft the tanners, and perhaps it would become lefs permeable to water.

Thefe are only conjectures which I advance, as they appear to me to be dictated by the theory of the art of tanning. In this inftance probably, as in many others, the practice will not correfpond with the theory. I recommended the trial to manufacturers, and fhall make it myfelf when a favourable opportunity offers. *Annales de Chimie, No.* 123.

V.

On the Deftruction of the Grub of the Cock-chafer. By EDWARD JONES, *Efq. of Wepre-Hall, in Flintfhire* *.

THE grubs of the cock-chafers (or brown beetles) are white, about an inch in length, and of the thicknefs of a turkey's quill. When difturbed they contract their length, and their bodies dilating, appear like lumps of white fat †, fomewhat oval. Defcription of the grub of the cockchafer.

They inhabit fandy and light loamy foils, lie from about two to fix inches deep, and may be found in fpring, by paring off the fods. Situation.

This place was much infefted by brown beetles; but about twelve years ago, fome labourers removing a bank of earth, expofed a bed of grubs, feveral paces in length. Many of them were fcattered among the fallen foil; and one of the men propofed to ftrip the furface of the bank, which being done, the grubs were feen lying in irriguous channels, as if the parent infects had dropped the eggs moving in various directions.

* Extracted from a letter to the Society for the Encouragement of Arts. Tranf. for 1801.

† Hence the Britifh name "Earth-Lard."

The

They are the favourite food of moles,

The ſame man informed me that they were the favourite food of moles; and he deſired me to obſerve an end of the bank not ſtripped (being covered with mole hills); " for there no beetle grubs would be found." When opened, his remark proved true:—the moles had traced all the labyrinths of the grubs.

which on this account deſerves protection.

I took the hint for the preſervation of my foliage, and have ever ſince protected the race of moles. The brown beetles gradually decreaſed, and are now rarely ſeen here. I have not obſerved more than one or two ſtragglers in the two laſt ſprings.

General habits of moles.

Some notice of the habits of moles may be acceptable to the Society, as it has been ſaid " that they penetrate deep into the earth in dry weather; rarely quit their ſubterraneous dwellings, aud have few enemies;"—and " that they do great miſchief in gardens and corn-grounds."

They are much on the ſurface while the graſs is high.

I have always found that in hay and paſture grounds, as ſoon as the graſs is high enough to cover them, they run upon the ſurface, where they find their food in the numerous caterpillars and inſects which in the early part of the ſummer crawl out of the earth; and they continue above ground till the harveſt. They are frequently cut by the ſcythe; and I have ſeen them at various times come out of deep hay graſs into places recently mown, and, perceiving their expoſure, endeavour to conceal themſelves in the ſhorn graſs.

Do not dig deep;

I have alſo often ſeen moles on very cloſe mown graſs, and bare ſpots in paſture land, plunge, when alarmed, among the roots; following their path (which was diſcernible by the heaving of the ſurface), I have forced them out occaſionally, to try the depth of the covering, which was only a few ſhreds of roots.

except to avoid the plough or ſpade, &c.

There are two circumſtances that may oblige moles ſometimes to penetrate deeply:—diſturbed ſoils in ſummer, ſuch as in gardens; and ploughed light lands, where the moles delve in purſuit of worms; and, in their courſe, they muſt unroot and deſtroy ſome plants; but a vigilant gardener and huſbandman will prevent much damage.

or to eſcape froſt, &c.

The other cauſe of their digging deep is froſt, which they avoid, or it would kill them. I have found them in winter, in peat ſoil, two and three feet below the ſurface; and in the hard froſt of 1794-5 (cutting deep trenches to ſeparate grounds,)

grounds), I found moles ſeveral mornings, that had worked through and fallen into the trenches, frozen to death.

Their ſummer emerſion is proved by the birds of prey: they deſtroy great numbers of moles. This year there were taken out of one kite's neſt twenty-two moles, and out of another fifteen, ſome of which were putrid; beſides many frogs and unfledged birds.

Birds of prey deſtroy great numbers while above ground.

The rapacity of the kites ſhews that they are deſtructive enemies to the moles, which, if moles are ſerviceable to man, ſhould be known, that he may ſtay his arm.

Moles are frequently found dead upon the graſs in ſummer, with marks of having been bitten, as if to ſuck their blood, but with no part of their bodies conſumed. This, I ſuppoſe, is done by weaſels; and the following (not very common) occurrence, which happened in the ſummer of 1789, tends to prove it:—

A kite was obſerved riſing from the ground with ſome prey, and inſtead of flying to an adjoining wood, he ſoared almoſt perpendicularly. After remaining a ſhort time ſtationary, he came gradually down, with his wings extended and motionleſs, and dropt very near the place from which he had riſen *.

Incident of a kite deſtroyed by a weazel, &c.

Several perſons who were near, and ſaw the flight and deſcent, ran immediately to the ſpot, and a weaſel darted from the kite, which they found dead; and they diſcovered, on examination, that the kite had been bit in the throat, and bled to death. Near it they found a dead mole, yet warm, which was bitten in the neck; and they concluded that the weaſel had cauſed the death of both.

In ſeveral parts of the kingdom where I have met with a great number of brown beetles, moles were regularly deſtroyed; and in Staffordſhire, being ſhown ſeveral large trees covered by beetles, and totally defoliated, I enquired whether they deſtroyed the moles? The anſwer was, that they did, or they ſhould be over-run with them.

Brown beetles are abundant where the moles are deſtroyed.

The loſs of foliage not being of great conſequence to the farmer, he is ſatisfied that his turkeys make him amends in other reſpects, by eating the brown beetles, of which they are very fond, and which they ſwallow voraciouſly.

Other remarks.

* A ſimilar circumſtance was mentioned in the Cheſter papers, three or four years ago.

A gen-

A gentleman informed me lately, that rooks alſo eat the beetles.

But theſe means are confined to the winged beetle. It appears to me that the mole is the only certain deſtroyer of the grub.

My hay and paſture grounds are, every ſpring, thickly ſtudded with mole hillocks. They are ſcattered in the uſual manner; and when the graſſes are up, the moles ceaſe to work, and ſcarce a hillock appears till after harveſt.

VI.

Methods of diminiſhing the Irregularities of Time-Pieces, ariſing from differenees in the Arc of Vibration of the Pendulum. By Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Lynn Regis, May 17, 1802,

Chronometers, &c. rendered irregular by the oil.

AFTER all the improvements which the mechaniſm of clocks and watches has received, there are ſtill obſtacles which ſtand in the way of an exact performance. The uſe of oil in chronometers has long been complained of, but ſtill remains a neceſſary evil; and that variation which obtains in the arc of vibration of the pendulum, is a ſource of much error in clocks.

Tranſit clock at Greenwich varies in its arc of vibration.

The tranſit clock at Greenwich ſometimes varies in its ſemi-arc of vibration *twenty* minutes in a year, and a compound pendulum which came under my own inſpection ſome years ago, varied in its arc nearly as much.

Old remedy for the inequality of the arcs.

Many years have elapſed ſince it was diſcovered, that the ſhort vibrations were performed in leſs time than the long ones, and methods were uſed to remove the inconvenience. HUYGENS propoſed a method by which the centre of oſcillation might be made to vibrate in the arc of a cycloid, and demonſtrated that a pendulum moving in that curve, would perform all its vibrations, whether long or ſhort, in equal times: and others propoſed to remove the evil by a peculiar form of the pallats.

Theſe

Thefe contrivances, how ingenious foever they may appear in theory, have not anfwered any good purpofe in practice; but, what may be impracticable to perform by one method, may not be found fo by another. *Of little practical utility.*

The fpring by which the pendulum is fufpended feems capable of receiving fuch improvement, as would render the long and fhort vibrations ifochronal, without any other alteration in the mechanifm of the clock. This improvement confifts in cutting the fpring broader at the top than at the bottom, that it may not bend totally at the point of fufpenfion. The pendulum would, in that cafe, when in motion, become fhorter at the ends of the arc, confequently it would no longer vibrate in the arc of a circle, and on this account there is fome reafon for fuppofing that the long vibrations would be performed in lefs time than they were before. *Propofed remedy by accommodating the flexibility of the fpring of fufpenfion.*

When this method is put in practice, the fpring fhould be made fo broad at the point of fufpenfion at firft, that the long vibrations may be performed in lefs time than the fhort ones, and then made narrower till all the vibrations be performed in equal times. But I fuppofed that this method would be attended with much trouble, and that it would be better *to remove the caufe than to counteract its effects.* *Method of adjuftment.*

When I made obfervations on the pendulum above-mentioned, I found that it was much influenced by the weather. In cold and moift weather it moved through a larger arc, than when the atmofphere was dry and warm. *Influencing caufe,*

As all metals alter their dimenfions, and confequently their elaftic force by heat and cold, it follows that that part of the clock called the *crutch*, would act more forcibly on the pendulum in cold weather than in warm; and it feems probable, that this variation would be greater in a weak *crutch* than in a ftrong one. *fuppofed to operate by the crutch.*

About feven years ago having a regulator made by Mr. James Bullock, of Holborn, and being defirous of putting my theory to the teft of experience, I had the *crutch* made exceedingly ftrong, and firmly connected with the pallats, that the maintaining power of the clock might be communicated to the pendulum uniformly in all ftates of the atmofphere. The clock cafe is made very thick of folid mahogany, and upon the *rifing board* *, which is two inches thick, ftands an an- *Experiment with a regulator.*

* The board on which the wheel-work ftands.

gular

gular piece of braſs (thus Λ) to ſupport the pendulum. This ſupport is made faſt with ſcrews to the *riſing-board* at the bottom, and to the *frame-plate* of the clock at the top. Who invented this ſupport I know not, but it appears a very excellent method of giving ſtability to the point of ſuſpenſion of the pendulum.

Reſult.

This clock has now been going near ſeven years. It vibrates 1° 49′ on each ſide of the perpendicular, from which I have not ſeen it vary more than 2′, except once in a very hard froſt, and as it ſeldom varies ſo much as 2′ in its ſemi-arc, this cauſe of error in the pendulum ſeems to be very nearly removed. It has been cleaned only once ſince it came into my poſſeſſion, but this made no alteration, either in its arc of vibration or its rate of going.

I am, with much reſpect,

Sir,

Your very humble ſervant,

EZEKIEL WALKER.

VII.

On the Theory of Light and Colours. By* THOMAS YOUNG, *M. D. F. R. S. Profeſſor of Natural Philoſophy in the Royal Inſtitution.*

Importance of general principles in ſcience.

ALTHOUGH the invention of plauſible hypotheſes, independent of any connection with experimental obſervations, can be of very little uſe in the promotion of natural knowledge; yet the diſcovery of ſimple and uniform principles, by which a great number of apparently heterogeneous phenomena are reduced to coherent and univerſal laws, muſt ever be allowed to be of conſiderable importance towards the improvement of the human intellect.

Object of the preſent diſſertation.

The object of the preſent diſſertation is not ſo much to propoſe any opinions which are abſolutely new, as to refer ſome theories, which have been already advanced, to their original inventors, to ſupport them by additional evidence, and to apply them to a great number of diverſified facts, which have hitherto been buried in obſcurity. Nor is it abſolutely ne-

* In ſupport of the truth of that hypotheſis, which aſcribes the phenomena to undulatory motions of an extremely elaſtic and rare fluid. From the Philoſ. Tranſ. 1802.—N.

ceſſary

ceſſary in this inſtance to produce a ſingle new experiment; for of experiments there is already an ample ſtore, which are ſo much the more unexceptionable, as they muſt have been conducted without the leaſt partiality for the ſyſtem by which they will be explained; yet ſome facts, hitherto unobſerved, will be brought forwards, in order to ſhew the perfect agreement of that ſyſtem with the multifarious phenomena of nature.

Excellence of the optical obſervations of Newton.

The optical obſervations of Newton are yet unrivalled; and, excepting ſome caſual inaccuracies, they only riſe in our eſtimation, as we compare them with later attempts to improve on them. A further conſideration of the colours of thin plates, as they are deſcribed in the ſecond book of Newton's optics, has converted that prepoſſeſſion which I before entertained for the undulatory ſyſtem of light, into a very ſtrong conviction of its truth and ſufficiency; a conviction which has been ſince moſt ſtrikingly confirmed, by an analyſis of the colours of ſtriated ſubſtances. The phenomena of thin plates are indeed ſo ſingular, that their general complexion is not without great difficulty reconcileable to any theory, however complicated, that has hitherto been applied to them; and ſome of the principal circumſtances have never been explained by the moſt gratuitous aſſumptions; but it will appear, that the minuteſt particulars of theſe phenomena are not only perfectly conſiſtent with the theory which will now be detailed, but that they are all the neceſſary conſequences of that theory, without any auxiliary ſuppoſitions; and this by inferences ſo ſimple, that they become particular corollaries, which ſcarcely require a diſtinct enumeration.

Phenomena of thin plates.

Newton firſt ſuggeſted the theory of undulation.

A more extenſive examination of Newton's various writings has ſhown me, that he was in reality the firſt that ſuggeſted ſuch a theory as I ſhall endeavour to maintain; that his own opinions varied leſs from this theory than is now almoſt univerſally ſuppoſed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a ſimilar form in his own works; and this by no leſs a mathematician than Leonard Euler, whoſe ſyſtem of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huygens, and Malebranche.

Euler.

Reference to the works of Newton.

Thoſe who are attached, as they may be with the greateſt juſtice, to every doctrine which is ſtamped with the Newtonian approbation, will probably be diſpoſed to beſtow on theſe

conſideration

confiderations fo much the more of their attention, as they appear to coincide more nearly with Newton's own opinions. For this reafon, after having briefly ftated each particular pofition of my theory, I fhall collect, from Newton's various writings, fuch paffages as feem to be the moft favourable to its admiffion; and, although I fhall quote fome papers which may be thought to have been partly retracted at the publication of the optics, yet I fhall borrow nothing from them that can be fuppofed to militate againft his maturer judgment.

HYPOTHESIS I.

A luminiferous Ether pervades the Univerfe, rare and elaftic in a high degree.

Paffages from Newton.

Hypoth. I. Univerfality of the ether.

" The hypothefis certainly has a much greater affinity with " his own," that is, Dr. Hooke's, " hypothefis, than he feems " to be aware of; the vibrations of the ether being as ufeful " and neceffary in this as in his." (Phil. Tranf. Vol. VII. p. 5087. Abr. Vol. I. p. 145. Nov. 1672.)

" To proceed to the hypothefis: firft, it is to be fuppofed " therein, that there is an ethereal medium, much of the fame " conftitution with air, but far rarer, fubtler, and more ftrongly " elaftic. It is not to be fuppofed, that this medium is one " uniform matter, but compounded, partly of the main phleg" matic body of ether, partly of other various ethereal fpirits, " much after the manner that air is compounded of the phleg" matic body of air, intermixed with various vapours and ex" halations: for the electric and magnetic effluvia, and gravi" tating principle, feem to argue fuch variety." (Birch. Hift. of R. S. Vol. III. p. 249. Dec. 1675.)

Arguments propofed by Newton in fupport of the ether.

" Is not the heat (of the warm room) conveyed through the " vacuum by the vibrations of a much fubtiler medium than " air? And is not this medium the fame with that medium by " which light is refracted and reflected, and by whofe vibra" tions light communicates heat to bodies, and is put into fits " of eafy reflection, and eafy tranfmiffion? And do not the vi" brations of this medium in hot bodies, contribute to the in" tenfenefs and duration of their heat? And do not hot bodies " communicate their heat to contiguous cold ones, by the vi" brations of this medium propagated from them into the cold " ones?

"ones? And is not this medium exceedingly more rare and "subtile than the air, and exceedingly more elaftic and active? "And doth it not readily pervade all bodies? And is it not, "by its elaftic force, expanded through all the heavens?— "May not planets and comets, and all grofs bodies, perform "their motions in this ethereal medium? And may not its re- "fiftance be fo fmall, as to be inconfiderable? For inftance, if "this ether (for fo I will call it) fhould be fuppofed 700,000 "times more elaftic than our air, and above 700,000 times "more rare, its refiftance would be about 600,000,000 lefs "than that of water. And fo fmall a refiftance would fcarce "make any fenfible alteration in the motions of the planets, in "ten thoufand years. If any one would afk how a medium "can be fo rare, let him tell me, How an electric body can by "friction emit an exhalation fo rare and fubtile, and yet fo "potent? And how the effluvia of a magnet can pafs through "a plate of glafs without refiftance, and yet turn a magnetic "needle beyond the glafs?" (Optics, Qu. 18, 22.)

HYPOTHESIS II.

Undulations are excited in this Ether whenever a body becomes luminous.

Scholium. I ufe the word undulation, in preference to vibration, becaufe vibration is generally underftood as implying a motion which is continued alternately backwards and forwards, by a combination of the momentum of the body with an accelerating force, and which is naturally more or lefs permanent; but an undulation is fuppofed to confift in a vibratory motion, tranfmitted fucceffively through different parts of a medium, without any tendency in each particle to continue its motion, except in confequence of the tranfmiffion of fucceeding undulations, from a diftinct vibrating body; as, in the air, the vibrations of a chord produce the undulations conftituting found.

Hypoth. II. Light confifts in ethereal undulation.

Paffages from Newton.

"Were I to affume an hypothefis, it fhould be this, if propounded more generally, fo as not to determine what light "is, further than that it is fomething or other capable of exciting vibrations in the ether; for thus it will become fo general and comprehenfive of other hypothefes, as to leave "little room for new ones to be invented." (Birch. Vol. III. p. 249. Dec. 1675.)

Detail of the hypoth. by Newton. Light.

Vibrations compared with those of sound.

"In the second place, it is to be supposed, that the ether is "a vibrating medium like air, only the vibrations far more "swift and minute; those of air, made by a man's ordinary "voice, succeeding one another at more than half a foot, or a "foot distance; but those of ether at a less distance than the "hundred thousandth part of an inch. And, as in air the vi-"brations are some larger than others, but yet all equally swift, "(for in a ring of bells the sound of every tone is heard at two "or three miles distance, in the same order that the bells are "struck), so, I suppose, the ethereal vibrations differ in big-"ness, but not in swiftness. Now, these vibrations, beside "their use in reflection and refraction, may be supposed the "chief means by which the parts of fermenting or putrifying "substances, fluid liquors, or melted, burning, or other hot "bodies, continue in motion." (Birch. Vol. III. p. 251. Dec. 1675).

—— reflection, refraction;

"When a ray of light falls upon the surface of any pellucid "body, and is there refracted or reflected, may not waves of "vibrations, or tremors, be thereby excited in the refracting "or reflecting medium? And are not these vibrations propa-"gated from the point of incidence to great distances? And "do they not overtake the rays of light, and by overtaking "them successively, do not they put them into the fits of easy "reflection and easy transmission described above?" (Optics, Qu. 17).

and the alternate fits.

"Light is in fits of easy reflection and easy transmission, be-"fore its incidence on transparent bodies. And probably it is "put into such fits at its first emission from luminous bodies, "and continues in them during all its progress." (Optics, Second Book, Part III. Prop. 13.)

HYPOTHESIS III.

The Sensation of different Colours depends on the different frequency of Vibrations, excited by Light in the Retina.

Passages from Newton.

Hypoth. III. Colour depends on the frequency of the ethereal vibrations.

"The objector's hypothesis, as to the fundamental part of it, is not against me. That fundamental supposition is, that the parts of bodies, when briskly agitated, do excite vibrations in the ether, which are propagated every way from those bodies in straight lines, and cause a sensation of light by beating and

and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing. Now, the most free and natural application of this hypothesis to the solution of phenomena, I take to be this: that the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the ether of various depths or bignesses, which, being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal bignesses be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours; much after the mannner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various bignesses, which, according to those bignesses, make several tones in sound: that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction; whence the vibrations of several bignesses, that is, the rays of several colours, which are blended together in light, must be parted from one another by refraction, and so cause the phenomena of prisms, and other refracting substances; and that it depends on the thickness of a thin transparent plate or bubble, whether a vibration shall be reflected at its further superficies, or transmitted; so that, according to the number of vibrations, interceding the two superficies, they may be reflected or transmitted for many successive thicknesses. And, since the vibrations which make blue and violet, are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate: which is sufficient to explicate all the ordinary phenomena of those plates or bubbles, and also of all natural bodies, whose parts are like so many fragments of such plates. These seem to be the most plain, genuine, and necessary conditions of this hypothesis. And they agree so justly with my theory, that if the animadversor think fit to apply them, he need not, on that account, apprehend a divorce from it. But yet, how he will defend it from other difficulties, I know not." (Phil. Transf. Vol. VII. p. 5088. Abr. Vol. I. p. 145. Nov. 1672.)

Particular developement;

as in sound.

Various refrangibility;

Thin transparent plates.

"To explain colours, I suppose, that as bodies of various sizes, densities, or sensations, do by percussion or other action excite

Repetition of the theory.

excite ſounds of various tones, and conſequently vibrations in the air of different bigneſs; ſo the rays of light, by impinging on the ſtiff refracting ſuperficies, excite vibrations in the ether, of various bigneſs; the biggeſt, ſtrongeſt, or moſt potent rays, the largeſt vibrations; and others ſhorter, according to their bigneſs, ſtrength, or power: and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being ſuch refracting ſuperficies, when the rays impinge upon them, they muſt there excite theſe vibrations, which vibrations (like thoſe of ſound in a trunk or trumpet) will run along the aqueous pores or cryſtalline pith of the capillamenta, through the optic nerves, into the ſenſorium; and there, I ſuppoſe, affect the ſenſe with various colours, according to their bigneſs and mixture; the biggeſt with the ſtrongeſt colours, reds and yellows; the leaſt with the weakeſt, blues and violets; the middle with green; and a confuſion of all with white, much after the manner that, in the ſenſe of hearing, nature makes uſe of aerial vibrations of ſeveral bigneſſes, to generate ſounds of divers tones; for the analogy of nature is to be obſerved." (Birch, Vol. III. p. 262, Dec. 1675.)

"Conſidering the laſtingneſs of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the moſt refrangible rays excite the ſhorteſt vibrations, —the leaſt refrangible the largeſt? May not the harmony and diſcord of colours ariſe from the proportions of the vibrations propagated through the fibres of the optic nerve into the brain, as the harmony and diſcord of ſounds ariſe from the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.)

Scholium. The parts of the retina being probably capable of vibrating in uniſon with a limited number of colorific motions,

Scholium. Since, for the reaſon here aſſigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations muſt be dependent on the conſtitution of this ſubſtance. Now, as it is almoſt impoſſible to conceive each ſenſitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect uniſon with every poſſible undulation, it becomes neceſſary to ſuppoſe the number limited, for inſtance, to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion leſs or more forcibly, by undulations

tions differing lefs or more from a perfect unifon; for inftance, the undulations of green light being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unifon with yellow and blue, and produce the fame effect as a light compofed of thofe two fpecies: and each fenfitive filament of the nerve may confift of three portions, one for each principal colour. Allowing this ftatement, it appears that any attempt to produce a mufical effect from colours, muft be unfuccefsful, or at leaft that nothing more than a very fimple melody could be imitated by them; for the period, which in fact conftitutes the harmony of any concord, being a multiple of the periods of the fingle undulations, would in this cafe be wholly without the limits of fympathy of the retina, and would lofe its effect; in the fame manner as the harmony of a third or a fourth is deftroyed, by depreffing it to the loweft notes of the audible fcale. In hearing, there feems to be no permanent vibration of any part of the organ.

It is not to be expected that colours can have a mufical effect.

The ear not permanently affected.

HYPOTHESIS IV.

All material Bodies have an attraction for the ethereal Medium, by means of which it is accumulated within their fubftance, and for a fmall Diftance around them, in a State of greater Denfity, but not of greater Elafticity.

Hypoth. IV. The denfity but not the elafticity of the ether, is greater within and near other bodies.

It has been fhewn, that the three former hypothefes, which may be called effential, are literally parts of the more complicated Newtonian fyftem. This fourth hypothefis differs perhaps in fome degree from any that have been propofed by former authors, and is diametrically oppofite to that of Newton; but, both being in themfelves equally probable, the oppofition is merely accidental; and it is only to be inquired which is the beft capable of explaining the phenomena. Other fuppofitions might perhaps be fubftituted for this, and therefore I do not confider it as fundamental, yet it appears to be the fimpleft and beft of any that have occurred to me.

Newton fuppofed the contrary.

PROPOSITION I.

All impulfes are propagated in a homogeneous elaftic Medium with an equable velocity.

Propof. I. Impulfe is propagated uniformly in an homogeneous elaftic medium.

Every experiment relative to found coincides with the obfervation already quoted from Newton, that all undulations are propagated through the air with equal velocity; and this is fur-

ther confirmed by calculations. (Lagrange, Misc. Taur. Vol. I. p. 91. Also, much more concisely, in my Syllabus of a course of Lectures on Natural and Experimental Philosophy, about to be published. Article 289.) If the impulse be so great as materially to disturb the density of the medium, it will be no longer homogeneous; but, as far as concerns our senses, the quantity of motion may be considered as infinitely small. It is surprising that Euler, although aware of the matter of fact, should still have maintained, that the more frequent undulations are more rapidly propagated. (Theor. mus. and Conject. phys.) It is possible, that the actual velocity of the particles of luminiferous ether may bear a much less proportion to the velocity of the undulations than in sound; for light may be excited by the motion of a body moving at the rate of only one mile in the time that light moves an hundred millions.

Law of the velocities in different mediums.

Scholium 1. It has been demonstrated, that in different mediums the velocity varies in the subduplicate ratio of the force directly, and of the density inversely. (Misc. Taur. Vol. I. p. 91. Young's Syllabus. Art. 294.)

Undulations do not mix.

Scholium 2. It is obvious, from the phenomena of elastic bodies and of sounds, that the undulations may cross each other without interruption. But there is no necessity that the various colours of white light should intermix their undulations; for, supposing the vibrations of the retina to continue but a five hundredth of a second after their excitement, a million undulations of each of a million colours may arrive in distinct succession within this interval of time, and produce the same sensible effect, as if all the colours arrived precisely at the same instant.

PROPOSITION II.

Prop. II. Nature of undulation.

An Undulation conceived to originate from the Vibration of a single Particle, must expand through a homogeneous Medium in a spherical Form, but with different Quantities of Motion in different Parts.

For, since every impulse, considered as positive or negative, is propagated with a constant velocity, each part of the undulation must in equal times have past through equal distances from the vibrating point. And, supposing the vibrating particle, in the course of its motion, to proceed forwards to a small distance in a given direction, the principal strength of the undulation

ation will naturally be ſtraight before it; behind it, the motion will be equal, in a contrary direction; and, at right angles to the line of vibration, the undulation will be evaneſcent.

Now, in order that ſuch an undulation may continue its progreſs to any conſiderable diſtance, there muſt be in each part of it, a tendency to preſerve its own motion in a right line from the centre; for, if the exceſs of force at any part were communicated to the neighbouring particles, there can be no reaſon why it ſhould not very ſoon be equaliſed throughout, or, in other words, become wholly extinct, ſince the motions in contrary directions would naturally deſtroy each other. The origin of ſound from the vibration of a chord is evidently of this nature; on the contrary, in a circular wave of water, every part is at the ſame inſtant either elevated or depreſſed. It may be difficult to ſhow mathematically, the mode in which this inequality of force is preſerved; but the inference from the matter of fact, appears to be unavoidable; and, while the ſcience of hydrodynamics is ſo imperfect that we cannot even ſolve the ſimple problem of the time required to empty a veſſel by a given aperture, it cannot be expected that we ſhould be able to account perfectly for ſo complicated a ſeries of phenomena, as thoſe of elaſtic fluids. The theory of Huygens indeed explains the circumſtance in a manner tolerably ſatisfactory: he ſuppoſes every particle of the medium to propagate a diſtinct undulation in all directions; and that the general effect is only perceptible where a portion of each undulation conſpires in direction at the ſame inſtant; and it is eaſy to ſhow that ſuch a general undulation would in all caſes proceed rectilinearly, with proportionate force; but, upon this ſuppoſition, it ſeems to follow, that a greater quantity of force muſt be loſt by the divergence of the partial undulations, than appears to be conſiſtent with the propagation of the effect to any conſiderable diſtance. Yet it is obvious, that ſome ſuch limitation of the motion muſt naturally be expected to take place; for, if the intenſity of the motion of any particular part, inſtead of continuing to be propagated ſtraight forwards, were ſuppoſed to affect the intenſity of a neighbouring part of the undulation, an impulſe muſt then have travelled from an internal to an external circle in an oblique direction, in the ſame time as in the direction of the radius, and conſequently with a greater velocity; againſt the firſt

Theory of Huygens.

propoſition.

propoſition. In the caſe of water, the velocity is by no means ſo rigidly limited as in that of an elaſtic medium. Yet it is not neceſſary to ſuppoſe, nor is it indeed probable, that there is abſolutely not the leaſt lateral communication of the force of the undulation, but that, in highly elaſtic mediums, this communication is almoſt inſenſible. In the air, if a chord be perfectly inſulated, ſo as to propagate exactly ſuch vibrations as have been deſcribed, they will in fact be much leſs forcible than if the chord be placed in the neighbourhood of a ſounding board, and probably in ſome meaſure becauſe of this lateral communication of motions of an oppoſite tendency. And the different intenſity of different parts of the ſame circular undulation may be obſerved, by holding a common tuning fork at arm's length, while ſounding, and turning it, from a plane directed to the ear, into a poſition perpendicular to that plane.

PROPOSITION III.

Propoſition III. Lateral undulations explained.

A Portion of a ſpherical Undulation, admitted through an Aperture into a quieſcent Medium, will proceed to be further propagated rectilinearly in concentric Superficies, terminated laterally by weak and irregular Portions of newly diverging Undulations.

At the inſtant of admiſſion, the circumference of each of the undulations may be ſuppoſed to generate a partial undulation, filling up the naſcent angle between the radii and the ſurface terminating the medium; but no ſenſible addition will be made to its ſtrength by a divergence of motion from any other parts of the undulation, for want of a coincidence in time, as has already been explained with reſpect to the various force of a ſpherical undulation. If indeed the aperture bear but a ſmall proportion to the breadth of an undulation, the newly generated undulation may nearly abſorb the whole force of the portion admitted; and this is the caſe conſidered by Newton in the Principia. But no experiment can be made under theſe circumſtances with light, on account of the minuteneſs of its undulations, and the interference of inflection; and yet ſome faint radiations do actually diverge beyond any probable limits of inflection, rendering the margin of the aperture diſtinctly viſible in all directions; theſe are attributed by Newton to ſome unknown cauſe, diſtinct from inflection: (Optics, Third

Third Book, Obf. 5. and they fully anfwer the defcription of this propofition.

Let the concentric lines in Fig. 1. (Plate V.) reprefent the contemporaneous fituation of fimilar parts of a number of fucceffive undulations diverging from the point A; they will alfo reprefent the fucceffive fituations of each individual undulation: let the force of each undulation be reprefented by the breadth of the line, and let the cone of light A B C be admitted through the aperture B C; then the principal undulations will proceed in a rectilinear direction towards G H, and the faint radiations on each fide will diverge from B and C as centres, without receiving any additional force from any intermediate point D of the undulation, on account of the inequality of the lines D E and D F. But, if we allow fome little lateral divergence from the extremities of the undulations, it muft diminifh their force, without adding materially to that of the diffipated light; and their termination, inftead of the right line B G, will affume the form C H; fince the lofs of force muft be more confiderable near to C than at greater diftances. This line correfponds with the boundary of the fhadow in Newton's firft obfervation, Fig. 1; and it is much more probable that fuch a diffipation of light was the caufe of the increafe of the fhadow in that obfervation, than that it was owing to the action of the inflecting atmofphere, which muft have extended a thirtieth of an inch each way in order to produce it; efpecially when it is confidered that the fhadow was not diminifhed by furrounding the air with a denfer medium than air, which muft in all probability have weakened and contracted its inflecting atmofphere. In other circumftances, the lateral divergence might appear to increafe, inftead of diminifhing, the breadth of the beam.

Linear explanation.

As the fubject of this propofition has always been efteemed the moft difficult part of the undulatory fyftem, it will be proper to examine here the objections which Newton has grounded upon it.

Objections of Newton to the undulatory fyftem of light.

"To me, the fundamental fuppofition itfelf feems impoffible; namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in ftraight lines, without a continual and very extravagant fpreading and bending every way into the quiefcent medium, where they are terminated by it.

it. I miſtake, if there be not both experiment and demonſtration to the contrary." (Phil. Tranſ. VII. 5089, Abr. I. 146. Nov. 1672.)

"*Motus omnis per fluidum propagatus divergit a recto tramite in ſpatia immota.*"

"*Quoniam medium ibi,*" in the middle of an undulation admitted, "*denſius eſt, quam in ſpatiis hinc inde, dilatabit ſeſe tam verſus ſpatia utrinque ſita, quam verſus pulſuum rariora intervalla; eoque pacto—pulſus eadem fere celeritate ſeſe in medii partes quieſcentes hinc inde relaxare debent;—ideoque ſpatium totum occupabunt.—Hoc experimur in ſonis.*" (Princip. Lib. II. Prop. 42.)

"Are not all hypotheſes erroneous, in which light is ſuppoſed to conſiſt in preſſion or motion, propagated through a fluid medium?—If it conſiſted in preſſion or motion, propagated either in an inſtant, or in time, it would bend into the ſhadow. For preſſion or motion cannot be propagated in a fluid in right lines beyond an obſtacle which ſtops part of the motion, but will bend and ſpread every way into the quieſcent medium which lies beyond the obſtacle. The waves on the ſurface of ſtagnating water, paſſing by the ſides of a broad obſtacle which ſtops part of them, bend afterwards, and dilate themſelves gradually into the quiet water behind the obſtacle. The waves, pulſes, or vibrations of the air, wherein ſounds conſiſt, bend manifeſtly, though not ſo much as the waves of water. For a bell or a cannon may be heard beyond a hill, which intercepts the ſight of the ſounding body; and ſounds are propagated as readily through crooked pipes as ſtraight ones. But light is never known to follow crooked paſſages, nor to bend into the ſhadow. For the fixed ſtars, by the interpoſition of any of the planets, ceaſe to be ſeen. And ſo do the parts of the ſun, by the interpoſition of the moon, Mercury, or Venus. The rays which paſs very near to the edges of any body, are bent a little by the action of the body; but this bending is not towards but from the ſhadow, and is performed only in the paſſage of the ray by the body, and at a very ſmall diſtance from it. So ſoon as the ray is paſt the body, it goes right on." (Optics, Qu. 28.)

Anſwers to the objections of Newton.

Now the propoſition quoted from the Principia does not directly contradict this propoſition; for it does not aſſert that ſuch a motion muſt diverge equally in all directions; neither

can

can it with truth be maintained, that the parts of an elaſtic medium communicating any motion, muſt propagate that motion equally in all directions. (Phil. Tranſ. for 1800. p. 109—112.) All that can be inferred by reaſoning is, that the marginal parts of the undulation muſt be ſomewhat weakened, and that there muſt be a faint divergence in every direction; but whether either of theſe effects might be of ſufficient magnitude to be ſenſible, could not have been inferred from argument, if the affirmative had not been rendered probable by experiment.

Sound deflects leſs than waves of water.

As to the analogy with other fluids, the moſt natural inference from it is this: "The waves of the air, wherein ſounds "conſiſt, bend manifeſtly, though not ſo much as the waves "of water;" water being an inelaſtic, and air a moderately elaſtic medium; but ether being moſt highly elaſtic, its waves bend very far leſs than thoſe of the air, and therefore almoſt imperceptibly. Sounds are propagated through crooked paſſages, becauſe their ſides are capable of reflecting ſound, juſt as light would be propagated through a bent tube, if perfectly poliſhed within.

Crooked paſſages.

The light of a ſtar is by far too weak to produce, by its faint divergence, any viſible illumination of the margin o a planet eclipſing it; and the interception of the ſun's light by the moon, is as foreign to the qneſtion, as the ſtatement of inflection is inaccurate.

To the argument adduced by Huygens, in favour of the rectilinear propagation of undulations, Newton has made no reply; perhaps becauſe of his own miſconception of the nature of the motions of elaſtic mediums, as dependent on a peculiar law of vibration, which has been corrected by later mathematicians. (Phil. Tranſ. for 1800, p. 116.) On the whole, it is preſumed, that this propoſition may be ſafely admitted, as perfectly conſiſtent with analogy and with experiment.

(To be continued.)

VIII. *Remarks*

VIII.

Remarks on Combuſtion. *By* THOMAS THOMSON, *M. D.* *Lecturer on Chemiſtry in Edinburgh.*

(Concluded from Page 20.)

Nitric acid formed ſpontaneouſly;

VI. NITRIC acid is formed ſpontaneouſly upon the ſurface of the earth by proceſſes with which we are but imperfectly acquainted; but which certainly have no reſemblance to combuſtion. Its oxigen is probably furniſhed by the *air*, which is a ſupporter; at leaſt, it has been obſerved, that if azote, the inſtant it is evolved, comes in contact with air, it is capable of combining with its oxigen, and forming nitric acid.

and alſo by electricity, through common air, and probably by galvaniſm;

Nitric acid may be formed alſo, as Mr. Cavendiſh has demonſtrated, by paſſing electric ſparks through common air, a ſupporter. In all probability it may be formed alſo by the galvanic pile, but this may be conſidered as equivalent to electricity. This formation of nitric acid by means of electricity, has been conſidered as a combuſtion, but for what reaſon it is not eaſy to ſay: the ſubſtance acted upon is not a combuſtible with a ſupporter, but a ſupporter alone. Electricity is ſo far from being equivalent to combuſtion, that it ſometimes acts in a manner diametrically oppoſite; *unburning*, if I may uſe the expreſſion, a ſubſtance which has already undergone combuſtion, and converting a *product* into a *combuſtible* and a *ſupporter*. Thus it decompoſes water, and converts it into oxigen and hidrogen gas; therefore it muſt be capable of ſupplying the ſubſtances which the oxigen and combuſtible loſe when they combine by combuſtion, and form a product *.

Apparently by decombuſtion.

Nitric acid in the proceſs for compoſing water.

There is one proceſs more, during which nitric acid is formed, which muſt at firſt ſight appear an exception to the general rule; I mean the formation of nitric acid, which takes place during the combuſtion of hidrogen gas in oxigen gas contaminated with air. But in this caſe it is the *hidrogen* only which burns, and not the *air;* the air indeed combines intimately, and forms nitric acid, juſt as it does when electric ſparks are

* I do not mean to affirm that electricity never occaſions combuſtion, the contrary of which is well known, but that a combination produced by it is not always the ſame with combuſtion.

paſſed

paſſed through it. But this proceſs has no reſemblance to combuſtion. We ſee, however, that a certain temperature is capable of producing this change in air.

8. Several of the ſupporters and partial ſupporters are capable of combining with combuſtibles, without undergoing decompoſition, or exhibiting the phenomena of combuſtion. In this manner the yellow oxide of gold and the white oxide of ſilver combine with ammonia; the red oxide of mercury with oxalic acid; and oximuriatic acid with ammonia. Thus alſo nitre and oximuriate of potaſh may be combined, or at leaſt intimately mixed with ſeveral combuſtible bodies, as in gunpowder, &c. In all theſe compounds the oxigen of the ſupporter and the combuſtible retain the ingredients which render them ſuſceptible of combuſtion; hence the compound is ſtill combuſtible: And in conſequence of the intimate combination of the component parts, the leaſt alteration is apt to deſtroy the equilibrium which ſubſiſts between them; the conſequence is, combuſtion and the formation of a new compound. Hence theſe compounds burn with amazing facility, not only when heated, but when triturated or ſtruck ſmartly with a hammer. They have therefore received the name of *detonating* or *fulminating* bodies. Thus we have fulminating gold, fulminating ſilver, fulminating mercury, fulminating powder, &c.

Supporters, &c. may combine with combuſtibles without combuſtion, and produce *fulminating compounds*.

9. Such are the properties of the combuſtibles, the ſupporters, and the products; and ſuch the phenomena which they exhibit when made to act upon each other.

If we compare together the *ſupporters* and the *products*, we ſhall find that they reſemble each other in ſeveral reſpects. Both of them contain oxigen as an eſſential conſtituent part; both are capable of converting combuſtibles into products; and ſeveral of both combine with combuſtibles and with additional doſes of oxigen. But they differ widely from each other in the phenomena which accompany their action on combuſtibles. The ſupporters convert theſe bodies into products, and combuſtion, or the emiſſion of heat and light at the ſame time, takes place; whereas the products convert combuſtibles into products without any ſuch emiſſion. Now, as the ultimate change produced upon combuſtibles by both theſe ſets of bodies is the ſame, and as the ſubſtance which combines with the combuſtibles is in both caſes the ſame, namely oxigen, we

Supporters and products reſemble in many reſpects;

but they differ widely in their effect on combuſtibles. The former only produce combuſtion.

must conclude that this oxigen in the supporters contains something which the oxigen of the products wants, something which separates during the passage of the oxigen from the product to the combustible, and occasions the combustion, or emission of fire, which accompanies this passage. The oxigen of supporters then contains some ingredient which the oxigen of products wants. Many circumstances concur to render it probable that this ingredient is *caloric*.

The oxigen of *supporters* contains *caloric*.

Combustibles and products also resemble each other:

But the former *emit fire* when they combine with oxigen.

The *combustibles* and the *products* also resemble each other in several respects. Both of them contain the same or a similar base; both frequently combine with combustibles, and likewise with oxigen; but they differ essentially in the phenomena which accompany their combination with oxigen. In the one case *fire* is emitted, in the other not. If we recollect that no substance but a combustible is capable of restoring combustibility to the base of a product, and that at its doing so it always loses its own combustibility; and if we recollect farther, that the base of a product does not exhibit the phenomena of combustion even when it combines with oxigen, we cannot avoid concluding, that all combustibles contain an ingredient which they lose when converted into products and that this loss contributes to the fire which makes its appearance during the conversion. Many circumstances contribute to render it probable that this ingredient is *light*.

Combustibles contain *light*.

When supporters and combustibles combine, the *caloric and light* fly off in the *combination called fire*.

If we suppose that the oxigen of supporters contains caloric as an essential ingredient, and that light is a component part of all combustibles, the phenomena of combustion above enumerated, numerous and intricate as they are, admit of an easy and obvious explanation. The component parts of the oxigen of supporters are two; namely, 1. a base, 2. caloric: The component parts of combustibles are likewise two; namely, 1. a base, 2. light. During combustion the base of the oxigen combines with the base of the combustible, and forms the product; while at the same time the caloric of the oxigen combines with the light of the combustible, and the compound flies off in the form of fire. Thus combustion is a double decomposition; the oxigen and combustible divide themselves each into two portions, which combine in pairs; the one compound is the *product*, and the other the *fire*, which escapes.

Combustion will not ensue if either of these in-

Hence the reason that the oxigen of products is unfit for combustion. It wants its caloric. Hence the reason that combus-

tion

tion does not take place when oxigen combines with products or with the bafe of fupporters. Thefe bodies contain no light. The caloric of the oxigen of courfe is not feparated, and no fire appears. And this oxigen ftill retaining its caloric, is capable of producing combuftion whenever a body is prefented which contains light, and whofe bafe has an affinity for oxigen. Hence alfo the reafon why a combuftible alone can reftore combuftibility to the bafe of a product. In all fuch cafes a double decompofition takes place. The oxigen of the product combines with the bafe of the combuftible, while the light of the combuftible combines with the bafe of the product. Thus when iron acts on water, the oxigen of the water combines with the bafe of the iron, while at the fame time the *light* of the iron combines with the hidrogen of the water, and occafions its efcape in the ftate of gas.

gredients of fire be abfent.

But the application of this theory to all the different phenomena defcribed above, is fo obvious, that it is needlefs to give any more examples. Let us rather inquire into the evidences which can be brought forward in its fupport.

10. Now as caloric and light are always emitted during combuftion, it follows that they muft have been previoufly component parts either of the combuftible, or of the fupporter, or of both.

They muft therefore have previoufly exifted in the combuftible, the fupporter, or both.

That the oxigen of the fupporters contains either one or both of thefe fubftances, follows incontrovertibly from a fact already mentioned, namely, that the oxigen of products will not fupport combuftion, while that of fupporters will. Hence the oxigen of fupporters muft contain fomething which the oxigen of products wants, and this fomething muft be caloric, or light, or both.

That the oxigen of fome of the fupporters at leaft contains caloric as an ingredient, has been proved, I think, in a fatisfactory manner, by the experiments of Crawford, Lavoifier, and La Place. Thus the temperature of hot blooded animals is maintained by the decompofition of *air*. Now if the oxigen of one fupporter contain caloric, the fame ingredient muft exift in the oxigen of every fupporter, becaufe all of them are obvioufly in the fame ftate. Hence I conclude that the oxigen of every fupporter contains caloric as an effential ingredient.

References to facts: Caloric.

The light emitted during combuftion muft either proceed from the combuftible or the fupporter. Now that it proceeds from the combuftible muft appear pretty obvious, if we recollect

Light.

lect that the colour of the light emitted during combuſtion varies, and that this variation uſually depends, not upon the ſupporter, but upon the combuſtible. Thus carbonic acid burns with a blue flame, carbonated hidrogen with a white, and charcoal with a red; ſulphur with a blue or violet, zinc with a greeniſh white, and phoſphorus with a white.

Natural formation of combuſtibles and ſupporters: Vegetation.

The formation of combuſtibles in plants obviouſly requires the preſence and agency of light; for when plants vegetate in the dark, their carbon is not increaſed, nor is any oily or reſinous matter formed in them. The leaves of plants emit oxigen gas when expoſed to the ſun's rays, but never in the ſhade, or in the dark. Senebier has demonſtrated that this emiſſion is occaſioned by the decompoſition of carbonic acid. This acid, which is a product of combuſtion, is decompoſed by the leaves of plants aſſiſted by ſunſhine, and converted into oxigen gas and charcoal, a *ſupporter* and a *combuſtible*. This proceſs is exactly the reverſe of combuſtion, and muſt therefore reſtore the ſubſtances which had been loſt during combuſtion; that is to ſay, caloric and light. But the ſun's rays conſiſt of theſe two bodies. Thus we ſee why plants require ſunſhine. A part of vegetation conſiſts in decompoſing, or *unburning*, products, and converting them into ſupporters and combuſtibles; but for ſuch a converſion caloric and light are abſolutely neceſſary.

The ſame effects by art.

Beſides vegetation, we are acquainted with two other methods of unburning products, or of converting them into products and combuſtible; by expoſing them, in certain circumſtances, to the agency of *fire* or of *electricity*. The oxides of gold, ſilver, and mercury, when heated to redneſs, are decompoſed, oxigen gas is emitted, and the pure metal remains behind. In this caſe the neceſſary caloric and light muſt be furniſhed by the fire; a circumſtance which explains why ſuch reductions always require a red heat. When carbonic acid is made to paſs repeatedly over red-hot charcoal, it combines with a portion of charcoal, and is converted into carbonic oxide gas. If this gas be a combuſtible oxide, the baſe of the carbonic acid and its oxigen muſt have been ſupplied with light and caloric from the fire; but if it be a *partial combuſtible*, it is merely a compound of carbonic acid and charcoal: which of the two it is, remains ſtill to be aſcertained. Electricity decompoſes water, and converts it into oxigen gas and hidrogen gas; it muſt therefore ſupply the heat and the light which theſe bodies loſt when converted into a product.

Theſe

These facts, together with the exact correspondence of the theory given above with the phenomena of combustion, render it so probable, that I have ventured to propose it as an additional step towards a full explanation of the theory of combustion. Every additional experiment has served to confirm it more and more*. It even throws light upon many phenomena which have been hitherto considered as altogether anomalous, as will be evident from the following observations. Application of this theory to other facts.

II. In the year 1793, the associated Dutch chemists drew the attention of philosophers to a curious phenomenon which accompanies the formation of some of the sulphurets; a phenomenon previously noticed by Scheele; but which they first described in detail. When eight parts of copper filings, and three parts of flowers of sulphur are mixed together in a glass receiver, and the vessel placed upon burning coals, the mixture melts, a kind of explosion takes place, it becomes suddenly red hot, and a glow, like that of a piece of red hot charcoal fanned by bellows, rapidly pervades the whole. When this disappears, the mixture is found in the state of solid sulphuret of copper. Iron, lead, tin or zinc, may be substituted for copper. The experiment succeeds whether the vessel be filled with air, or with azotic, or hidrogen gas, or even with water or mercury. What is singular in this experiment is the glowing *red heat*, or the emission of *fire* which accompanies the combination of the sulphur and metal. This emission being the same which takes place during combustion, the process has been considered as a combustion, and stated as such by the German chemists, as an objection to Lavoisier's theory, which supposes that oxigen is a necessary agent in that process: while other philosophers have denied that this operation is a combustion, or that it has any resemblance to that process. Ignition produced by the fusion of sulphur with a metal,

The same emission of caloric and light, or of *fire*, takes place when melted sulphur is made to combine with pot-ash, or with lime, in a crucible or glass tube, and likewise when or of sulphur with potash or lime; or phosphorus with lime, &c.

* In the preceding enumeration of facts I have not taken notice of the modifications which the Lavoiserian theory has received from Hutton, Delametherie, Richter, and Brugnatelli; because I suppose them sufficiently known. Every one of these modifications agrees in some particulars with the theory given in this Paper, but differs from it in others.

 melted

melted phoſphorus is made to combine with lime heated nearly to redneſs. In all probability barytes and ſtrontian exhibit the ſame phenomenon when combined with melted ſulphur or phoſphorus; and ſome of the metals when combined with phoſphorus. In general then the emiſſion of *fire* accompanies the combination of melted ſulphur and phoſphorus, with ſeveral of the earths, fixed alkalies and metals, heated previouſly to a certain temperature.

Explanation. *Fuſed* ſulphur or phoſphorus contain *caloric*: metal. &c. contain *light*. They become *ſolid* in combination and give out *caloric* and *light*; i. e. fire.

To explain the phenomenon we have only to recollect, 1. That the ſulphur and phoſphorus are in the melted ſtate, and therefore contain caloric as an ingredient. 2. That the alkalies, earths and metals which produce the phenomenon in queſtion, contain light as an eſſential ingredient. 3. That the ſulphuret or phoſphuret formed is always in a ſolid ſtate; theſe three points once eſtabliſhed, the proceſs admits of a very ſimple explanation. The ſulphur or phoſphorus combines with the baſe of the metal, earth or alkali; while at the ſame time the *caloric* to which the ſulphur or phoſphorus owed its fluidity, combines with the *light* of the metal, earth, or alkali, and the compound flies off under the form of *fire*.

The proceſs reſembles combuſtion except in its product;

Thus the proceſs is exactly the ſame with combuſtion, excepting as far as regards the product. The melted ſulphur or phoſphorus acts the part of the *ſupporter,* while the metal, earth or alkali occupy the place of the *combuſtible.* The firſt furniſhes caloric, the ſecond light, while the baſe of each combines together. Hence we ſee that the baſe of ſulphurets and phoſphurets reſembles the baſe of products in being deſtitute of light, the formation of theſe bodies exhibiting the ſeparation of fire like *combuſtion,* but the product differing from a product of combuſtion in being deſtitute of oxigen, we may diſtinguiſh the proceſs by the title of *ſemi-combuſtion*; indicating by the term, that it poſſeſſes one half of the characteriſtic marks of combuſtion, but is deſtitute of the other half.

termed ſemi-combuſtion.

Facts to ſhew that potaſh and lime contain light.

The only part of this theory which requires proof is, that light is a component part of the earths and alkalies. But as potaſh and lime are the only bodies of that nature, which I am certain to be capable of exhibiting the phenomena of ſemi-combuſtion, the proofs muſt of neceſſity be confined to them. Now that *lime* contains light as a component part has been long known. Meyer and Pelletier obſerved long ago, that when water is poured upon quicklime not only heat but light is emitted. Light is emitted alſo abundantly when ſulphuric

 acid

acid is poured upon lime*. In both cafes a *femi-combuftion* takes place. The water and the acid being folidified give out *caloric*, while the quicklime gives out *light;* that lime during its calcination combines with light, and that light is a component part of quicklime is demonftrated by the following experiment, for which we are indebted to Scheele. Semi-combuftion of lime and water.

It is well known that fluor fpar (native fluate of lime) has the property of phofphorefcing ftrongly when heated, but that the experiment does not fucceed twice with the fame fpecimen. After it has been once heated fufficiently, no fubfequent heat will caufe it to phofphorate. Now phofphorefcence is merely the emiffion of light, light of courfe is a component part of fluor fpar, and heat has the property of feparating it. But the phofphorefcing quality of the fpar may be again recovered to it, or which is the fame thing, the light which the fpar had loft may be reftored by the following procefs. Decompofe the fluate of lime by fulphuric acid, and preferve the fluoric acid feparated. Boil the fulphate of lime thus formed with a fufficient quantity of carbonate of foda; a double decompofition takes place; fulphate of foda remains in folution, and carbonate of lime precipitates. Calcine this precipitate in a crucible till it is reduced to quicklime, and combine it with the fluoric acid to which it was formerly united. The fluor fpar thus regenerated phofphorefces as at firft. Hence the lime during its calcination muft have combined with light. Phofphorefcence of fluor fpar explained, reftored.

That potafh contains light, may be proved in the fame manner as the exiftence of that body in quicklime. Dizé has fhown that much light is emitted when fulphuric acid is poured upon potafh, but more when it is poured on the carbonate of potafh. Now as potafh is deprived of its carbonic acid by lime, it is obvious that the procefs muft be a double decompofition; the bafe of the lime combines with carbonic acid, while its light combines with the potafh. Light in potafh.

Thefe remarks on femi-combuftion might eafily be extended much farther. For it is obvious, that whenever a liquid combines with a folid containing light, and the product is a folid body, fomething analogous to femi-combuftion muft take place. Hence the reafon why water increafes the violence of combuftion when thrown fparingly into a common fire.

* Dizé Jour. de Phyf. 49, 177.

IX.

A Continuation of the Experiments and Obſervations on the Light which is ſpontaneouſly emitted from various Bodies; with ſome Experiments and Obſervations on ſolar Light, when imbibed by CANTON'S *Phoſphorus. By* NATHANIEL HULME, *M. D. F. R. S. and A. S.*

(Concluded from page 40.*)*

§ 5.

The Effects of carbonic Acid Gas or fixed Air on ſpontaneous Light.*

EXPERIMENTS.

Carbonic acid gas extinguiſhes ſpontaneous light; but it revives by common air.

Exp. 1. AT 10 P. M. a piece of freſh herring, weighing about three drams, was ſuſpended in a wide-mouthed ten-ounce phial, filled with carbonic acid gas, and cloſed with a cork and bladder. It was retained there for three ſucceſſive nights; but emitted no light.

Exp. 2. The ſame experiment was made with a piece of herring, which was beginning to be luminous. On the next evening, the illumination was found to be extinct: nevertheleſs the herring was ſtill kept in the gas for three nights longer, but did not become lucid.

Exp. 3. At 7 P. M. a piece of freſh mackerel was introduced above water, into a wide-mouthed bottle, holding 24 ounces, which was completely filled with carbonic acid gas, and ſupported by a tea-ſaucer that held about three ounces of water. On the ſecond night it was dark, and continued the ſame on the third. It was then expoſed to the influence of atmoſpherical air, and, on the next evening, it was pretty luminous, and likewiſe on the ſucceeding night.

Exp. 4. At 9 P. M. a cork, ſmeared with the luminous matter of a mackerel, was put into a five-ounce wide-mouthed phial, filled with carbonic acid gas, and then cloſed with a glaſs

* This gas was obtained from powdered chalk, or marble, and diluted ſulphuric acid.

ſtopple.

ftopple. It continued to ſhine pretty vividly for ſome little time; then the light gradually diminiſhed, ſo that at twelve, only a ſmall ſpark remained.

Exp. 5. At 10 P. M. another cork, illuminated with mackerel-light, was introduced above water, into 24 ounces of the gas; and its light was nearly extinct at twelve.

Exp. 6. At 8 P. M. a fragment of ſhining wood was put above water, into 24 ounces of the gas; and it had not been long there before the light diſappeared. It was then taken out, and expoſed to the action of atmoſpheric air, when its ſhining property ſoon returned.

Exp. 7. Another fragment of brightly ſhining wood was introduced above water, into the ſame quantity of the gas, at 10 P. M. and the light was extinguiſhed in the ſpace of an hour. After this, it was expoſed to the open air, and the light gradually revived.

Exp. 8. At 8 P. M. a luminous dead glow-worm was put above water into the gas; its glowing appearance gradually faded, and in a ſhort time became quite inviſible. It was then taken out, and the light, by degrees, re-appeared as vivid as before.

OBSERVATION.

This gas, we find, has alſo an extinguiſhing property, with reſpect to ſpontaneous light: but, in general, the light returns, if the object of experiment be taken out, and expoſed to the open air.

§ 6

*The Effects of ſulphurated hidrogen Gas * on ſpontaneous Light.*

EXPERIMENTS.

Exp. 1. At noon, a piece of a very freſh mackerel, with a bright eye, was introduced above water, into 24 ounces of this gas, and was retained therein for three ſucceſſive evenings, without emitting any light. It was then expoſed to atmoſpheric air; yet it continued dark on the two following nights:

Sulphurated hidrogen extinguiſhes ſpontaneous light more effectually and more permanently than carbonic acid.

* This gas was obtained from ſulphuret of potaſh and diluted muriatic acid.

nights: but, on the third, it was very luminous, and remained ſo on the fourth and fifth.

Exp. 2. The ſame experiment was then made with a piece of freſh herring, which was alſo kept in the above gas, for about three nights, without being luminous. After expoſure to common air, it did not emit any light during the firſt 24 hours. However, on the ſubſequent night, it began to ſhine, had a very bright light on the following evening, and continued ſhining for ſeveral ſucceeding nights.

Exp. 3. A cork, ſmeared with the luminous matter of a herring, was put above water, into 24 ounces of the gas; and the light was extinguiſhed in leſs than an hour. The experiment was repeated in the ſame gas, and with the ſame reſult.

Exp. 4. A cork, illuminated with mackerel light, was introduced into the ſame quantity of gas; and was dark in half an hour.

Exp. 5. A fragment of ſhining wood, being put into the gas, became dark in eight minutes. A ſecond piece became dark in five minutes. They were then taken out, and continued dark all that evening. On the next evening, one of the pieces was uncommonly lucid.

Exp. 6. At 10 P. M. another fragment of brightly ſhining wood was introduced above water, into 24 ounces of the gas, and was extinct at eleven. It was then expoſed to the open air; but there was no return of light that evening. On the following night, it was found pretty luminous.

Exp. 7. A finely ſhining dead glow-worm was next put above water, into this gas, and its light was quickly extinguiſhed. In a ſecond experiment, in the ſame gas, the light was much ſlower in its extinction. In both inſtances, after the inſect was withdrawn, and placed in atmoſpheric air, the light gradually revived.

OBSERVATION.

It is apparent, by theſe experiments, that ſulphurated hidrogen gas extinguiſhes ſpontaneous light much ſooner than carbonic acid gas, and that, in general, the light returns much more ſlowly, when the ſubject is expoſed to atmoſpheric air.

§ 7.

§ 7.

*The Effects of nitrous Gas * on ſpontaneous Light.*

EXPERIMENTS.

Exp. 1. A piece of freſh herring was introduced above water, into this gas, at 3 P. M. and remained there four nights, without emitting any light: it was then withdrawn, and expoſed to common air, for the ſpace of three nights; but did not become lucid.

Nitrous gas quickly and effectually extinguiſhes ſpontaneous light.

Exp. 2. The ſame experiment was made with a piece of herring beginning to be luminous; but its light was gradually extinguiſhed: it was detained in the gas for three nights, and taken out dark. It was then expoſed to the open air, for the three ſubſequent nights; but its ſhining appearance did not return.

Exp. 3. A cork with luminous matter, introduced above water, into this ſpecies of gas, had its light, in general, extinguiſhed in from 10 to 30 minutes; and, when taken into common air, its light very ſeldom re-appeared.

Exp. 4. Fragments of ſhining wood, above water, in nitrous gas, were likewiſe commonly rendered dark in a very ſhort ſpace of time, as in three or four minutes; ſometimes a fragment, if uncommonly luminous, would not be extinguiſhed in leſs than ſix or eight minutes; and very ſeldom would the light revive, on expoſing the wood to atmoſpherical air.

Exp. 5. A dead ſhining glow-worm being put above water, into this gas, its light was quickly extinquiſhed; but, after the inſect was taken into the common atmoſphere, the light gradually returned. The experiment was thrice repeated, and with the ſame reſult.

OBSERVATION.

This ſpecies of gas, we obſerve to have totally prevented the emiſſion of light, and to have quickly extinguiſhed that which had been emitted: likewiſe that the luminous objects which had been under its influence, (except the glow-worm) did not experience a revival of their light, when taken out, and kept for ſome time in common air.

* This gas was obtained from copper and diluted nitrous acid.

§ 8.

§ 8.

The Effects of a Vacuum on ſpontaneous Light.

EXPERIMENTS.

The light is extinguiſhed in vacuo, but beautifully reſtored by admiſſion of air.

Exp. 1. A piece of ſhining wood, of a moderate ſize, was put under the receiver of an air-pump, in a dark room; in proportion as the air was extracted, the light was gradually extinguiſhed, and at laſt reduced to a mere point, juſt viſible, owing moſt probably to a ſmall reſiduum of air, which is always left, even in the moſt perfect machine. Freſh air was then leiſurely admitted, and the light was immediately revived in a very beautiful manner. This experiment was frequently repeated, and always with the like effect.

Exp. 2. Some luminous matter of a herring, uncommonly bright, was ſmeared upon a piece of red blotting paper, and then ſubmitted to the operation of the air-pump. The light became fainter and fainter, as the incloſed air was withdrawn, and at laſt nearly vaniſhed; but brightened up as before, on the influx of freſh air. The experiment was repeated, and with the ſame reſult.

SECTION XII.

Experiments and Obſervations on ſolar Light, when imbibed by Canton's Phoſphorus.

§ 1.

The Effects of Heat on imbibed ſolar Light.

I. *The imbibed Light is rendered more vivid by a moderate Degree of Heat.*

EXPERIMENTS.

Canton's phoſphorus ſhines more when heated.

Exp. 1. Having prepared ſome Canton's phoſphorus, and expoſed it to the light of the ſun, it was carried into the dark laboratory, to ſeparate the illuminated parts from thoſe that remained dark. In doing which, ſome luminous fragments were placed upon the palm of the hand, and retained there for ſome time, when it was obſerved, that the warmth of the hand conſiderably increaſed the degree of light.

Exp. 2.

Exp. 2. Some fragments of this illuminated phoſphorus were put into a ſmall phial, which was then cloſed with a cork, and ſuſpended, by a ſtring, in a quart of water heated to about 126° ; by theſe means, the light was rendered much more vivid than before.

Exp. 3. Some other pieces of the illuminated phoſphorus were dropped ſeparately into a glaſs tube 32 inches long, and $\frac{7}{10}$ bore, filled with water at about 120°. The light of each piece became exceedingly bright, as ſoon as it entered the hot water ; and they all deſcended, very luminous, from the top to the bottom, ſome quickly and others ſlowly, according to their gravity, making a very pleaſing experiment.

Exp. 4. A large wooden bowl, about 12 inches wide, was next filled with water heated to about 110°, and then a quantity of illuminated phoſphorus, partly in the form of powder, and partly in pieces of different magnitudes, was ſcattered over the whole ſurface of the water; all which pieces fell, with increaſed ſplendour, to the bottom, where they preſerved their light for ſome time.

II. *The imbibed Light is extinguiſhed by a great Degree of Heat.* but too much heat extinguiſhes it.

Exp. 5. Some fragments of the phoſphorus, rendered luminous, were expoſed to a greater degree of heat, namely, by caſting them into a tin veſſel containing two pints of boiling water. They flaſhed with increaſed light, as ſoon as they came in contact with the water, fell precipitately to the bottom, in a lucid ſtate, and then were gradually extinguiſhed.

Exp. 6. *In which the degree of heat was ſtill increaſed.* A ſmall bar of iron, of about an inch ſquare, was made red-hot, and laid horizontally in the laboratory, until, by cooling, it nearly ceaſed to ſhine. Some pieces of illuminated phoſphorus were then put upon it in ſucceſſion, and the light, in a moment, glowed with uncommon luſtre, but was quickly after totally extinguiſhed *.

* Solar light, when received merely on a piece of white paper, may alſo be rendered more luminous by heat, and then extinguiſhed by it, as appears from an experiment made by the late Mr. B. Wilſon, whoſe book on phoſphori I had not ſeen before this Paper was drawn up.

III. *The*

Latent light is excluded from Canton's phofphorus by heat.

III. *The imbibed Light, after being in a latent State, is excited and rendered luminous by the Agency of Heat.*

Exp. 7. Some fmall pieces of the phofphorus, after having been illuminated, were depofited in the laboratory; when the light by degrees faded away, and became totally invifible. They were kept in this dark ftate for the fpace of ten days, and then placed one after another upon a heated bar of iron, as in the laft experiment, upon which they quickly became exceedingly luminous.

From an experiment made by the ingenious Mr. Canton, I obferve, that fome of his phofphorus, contained in glafs balls hermetically fealed, and heated in the above manner, gave a confiderable degree of light, after it had been kept in a ftate of darknefs more than fix months. Phil. Tranf. Vol. LVIII. page 342.

§ 2.

The Effects of Cold on imbibed Light.

EXPERIMENT.

Cold extinguifhes the folar light of Canton's phofphorus, &c.

About 15 grains of the phofphorus were put into a half-ounce phial, containing two drams of cold pump water, that had been deprived of its air by boiling. The phial was then corked, and expofed for fome time to folar light, whereby the phofphorus became finely illuminated. In this ftate, it was immediately put into a frigorific mixture, compofed of fnow and fea falt, and retained there about 30 or 40 minutes, when it was taken out, and the light found to be totally extinguifhed. The phial was then placed in fome water, at about 60° temperature, and the light gradually revived, and became as brilliant as before it had been expofed to the cold. This experiment was frequently repeated, and always with the fame refult.

I cannot but remark, that in the courfe of experiments on this fubject, the fuperior power of folar over that of fpontaneous light was very apparent. For, the firft trials being made in fmall phials, containing only atmofpheric air with the phofphorus, the light was with fome difficulty totally extinguifhed; and, after the phials were taken out of the frigorific mixture,

mixture, the temperature of the laboratory would commonly ſoon revive the light, which rendered the experiments not altogether ſatisfactory. Finding it thus ſomewhat difficult to extinguiſh ſolar light *in air,* recourſe was had *to water,* in the manner above deſcribed. This anſwered perfectly well; for the water, when frozen, gave a ſubſtantial body, as it were, to the imbibed light of the phoſphorus, ſo as to enable it to retain the exceſs of cold ariſing from the frigorific mixture; thereby making the experiments quite ſatisfactory. When the phoſphorus was thus ſurrounded by ice, only a few minutes ſtay in the frigorific mixture would generally be ſufficient for a total extinction.

OBSERVATION.

From theſe experiments, compared with thoſe recited in my former Paper on ſpontaneous light, it appears that ſolar light, when imbibed by Canton's phoſphorus, is ſubject to the ſame laws, with reſpect to heat and cold, as the ſpontaneous light of fiſhes, rotten wood, and glow-worms.

P. S. In theſe experiments with ſolar light, the phoſphorus was ſometimes expoſed to the direct rays of the ſun, at other times to common day-light, in a northern aſpect; and it was remarked, that it became ſomewhat more luminous by mere day-light, than by the rays of the ſun.

Improved preparation of Canton's phoſphorus by Dr. Higgins.

It may alſo be proper to obſerve, that the above experiments were made with an improved preparation of Canton's phoſphorus. This improvement, which was firſt made by Dr. Higgins, conſiſts in omitting the pulverization of the ſhells. His method was, after calcining the oyſter-ſhells, to put the pieces, both great and ſmall, in layers, into a crucible furniſhed with a cover, and to ſprinkle flowers of ſulphur between each layer. After they had remained ſome time in the furnace, they were taken out, ſuffered to cool, and then kept in a large bottle with a glaſs ſtopple. For this communication, I am indebted to Mr. Lewis of Holborn, near Southampton-ſtreet, who has an extraordinary dark room, where, at times, he amuſes his friends with ſome beautiful appearances, ariſing from ſolar light imbibed by phoſphorus prepared as above directed. A ſtill further improvement of this phoſphorus, it appears to me, may be made by ſubſtituting precipitated ſulphur for the flowers of ſulphur; and the experiments of this ſection were chiefly made with phoſphorus ſo prepared.

Dark room of Mr. Lewis.

X. *Deſcription*

X.

Defcription of a Lamp upon ARGAND'S *Principle, with Improvements, in which the Oil is maintained at the fame Level by the conftant Action of a Pump. By Citizens* CARCEL and CAREAU *.

Excellence of the lamp of Argand.

LAMPS with a double current of air are among the inventions of the eighteenth century, which are moft honourable to the induftry of France, and of which the general ufe fufficiently befpeaks the value. But it is not enough that a great light fhould be produced without fmell or fmoke, but an object of nearly equal utility confifts in producing the light fteadily, with economy, in the moft advantageous form, and beft adapted to economical purpofes.

Improved by Carcel and Careau.

Citizens Carcel and Careau have fucceeded in this object. We fhall not dwell upon the obftacles they muft have met with in arriving at the degree of perfection they have obtained; it will be fufficient to fhew the value of their invention, if we can prove that it furpaffes every thing of the kind which has yet been exhibited. This lamp has the double advantage of exhibiting all the good qualities of the lamp we before poffeffed, without their inconveniences. The nozle undergoes no alteration by heat, neither is the wick deftroyed, but almoft conftantly preferves its whitenefs.

Inconveniences of lamps of the ufual conftruction.

The oil in ordinary lamps is liable to flow out by its expanfion when heated; but in this mechanical lamp it conftantly preferves its level. By difpenfing with the refervoirs of oil in thofe lamps which are called fountain lamps, thefe inventors have fucceeded in affording light which is not fhaded on any fide. In this refpect the lamp is very economical, becaufe nearly half the light of a fountain lamp muft neceffarily be intercepted by the receiver, which requires to be placed above the level. But in thefe the refervoir is in the foot, which renders them more portable; and from this circumftance, as well as the other advantages of their conftruction, there is no dan-

* From Les Annales des Arts & Manufactures, vi. 269. I am much obliged to my correfpondent C. D. for directing my attention to this article.

ger

ger of ſpilling the oil by inclining it, which is an inconvenience of great magnitude, and hitherto conſtituting one of the greateſt objections to the lamps in uſe.

In other lamps the flame varies in its intenſity commonly in about two hours; but in the mechanical lamp it conſtantly preſerves the ſame ſupply and the ſame brilliancy.

General remarks.

The mechaniſm adapted to the foot in order to cauſe the oil to aſcend, has been reduced to the greateſt ſimplicity; it is firm and durable, and has no communication with the oil of the reſervoir.

This lamp may be uſed in diſtillations and chemical preparations, as well as in culinary purpoſes; and in general we cannot do better than tranſcribe the report made to the National Inſtitute by Guyton, Morveau, and Charles.

Report to the Inſtitute of France.

"The Claſs having charged us to examine the mechanical lamp preſented by the Citizens Carcel and Careau, at the ſitting of the 21ſt of laſt month, as a means of adding a new degree of perfection to lamps with an inner current of air, as well as with regard to the intenſity of light as to economy and the convenience of daily uſe.

Hiſtory of the Argand lamp.

"Lamps excited by an interior current of air, of which the invention belongs to Citizen Argant, and which were announced for the firſt time in February 1784*, have produced a revolution in the art of illuminating which time has ſerved only to confirm, as it does all thoſe which, being founded on true principles, receive the daily ſanction of experience.

The chimney improved by Lange.

"A ſhort time afterwards Citizen Lange thought of contracting the glaſs chimney, ſo as to direct the external current of air nearer to the flame, by which means he determined a ſtill more complete combuſtion of the oil, and produced a more brilliant light, without either ſmoke or ſmell. The union of theſe inventions ſeemed to have exhauſted the ſubject, but Citizens Carcel and Careau apprehended that it was poſſible to render the lamps ſtill more perfect. They conſidered that the beſt lamps of this deſcription do not conſtantly afford the ſame intenſity of light, becauſe the wick not being conſtantly and alike ſupplied with oil, is ſubject to become charred; that it is neceſſary either to raiſe it beyond the proper elevation, or to trim the lamp again after ſome hours; and laſtly, that the glaſs

Improvements by Carcel and Careau.

* Journal de Phyſique for that month, page 159.

chimney

chimney not being capable of being made to that degree of accuracy as to afford the contraction always at the fame height, the effect of this contrivance could not be the beft poffible, excepting when the accidental concurrence of circumftances might render it fo.

" At the fame time that they were bufied in remedying thofe inconveniences, they did not neglect the means of rendering the lamp more economical, convenient, and portable; without fear of fpilling the oil; without any confiderable effect upon the flame by motion; without any interception of the light from a refervoir; and with the addition of an agreeable and ornamental form.

" The account refulting from our examination will enable the Clafs to judge of the difficulties they have had to vanquifh, the ingenious proceffes they have ufed, and the fuccefs which has crowned their induftry.

Experiments with the new lamp.

" The mechanical lamp was lighted before the Clafs for a fhort time. They faw the brilliant light it afforded, and compared that light with the lamps which ufually illuminate their place of meeting. It continued abfolutely the fame five hours after it had been lighted, though the wick had not been altered or touched.

Compared with the common, it gives more than twice the light.

" In order to determine more exactly the intenfity of the light, it was placed at fuch a diftance that a body interpofed between its light prodcced a fhadow of the fame obfcurity as that of a common Argand's lamp. The diftances were—from the mechanical lamp forty-fix decimeters, and from the common lamp thirty-three decimeters, which, by the fquare of the diftances, gives the ratio of 2116 to 1089, or 100 to $48\frac{3}{4}$, for the intenfities of the light.

" The common lamps of this conftruction not always affording a light perfectly uniform, and the glafs chimney of that which we employed not being of the moft favourable dimenfions, we thought it proper to endeavour to obtain a confirmation of this ratio by the comparifon with candles, of which the light is lefs fubject to vary.

Comparifon with candles.

" Six candles (I fuppofe of wax), of the weight of one hectogram (five to the French pound), were arranged in fuch a manner that their flames could not mutually intercept each other. The fhadow produced by the interpofition of an opaque body received on a card, was found to be fimilar to that of the mechanical

mechanical lamp when the candles were brought to the diſtance of 566 centimeters, at the ſame time that the mechanical lamp was drawn back to 785. The ſquares of theſe numbers give the ratio of 100 to 52 nearly. If inſtead of the latter term we put the quotient of its diviſion by the number of candles, we ſee that the light of the mechanical lamp is to that of one candle as 100 to $11\frac{1}{2}$, or it would require eleven candles and a half to give the ſame light.

"As this lamp may be uſed for the domeſtic purpoſes of affording heat as well as for the operations of chemiſtry even in the dry way, it becomes intereſting to aſcertain the degree of heat it was capable of communicating to veſſels placed above its glaſs chimney. For this purpoſe we took a pyrometric piece of Wedgwood not baked; it was included between two very thin capſules of platina, and placed on a ſupport, firſt at four centimeters, where, at the end of half an hour, the contraction was three degrees of the pyrometric ſcale; after which it was again placed at the diſtance of fifteen millimeters only from the upper extremity of the chimney, where, after having been kept for two hours, it was found to have paſſed the ſeventh degree, which, according to the table of correſpondence of Wedgwood, would indicate 505 degrees of the centigrade thermometer.

The heat it gives to other bodies placed over the flame.

Pyrometric piece.

"A tube of glaſs of five millimeters, or two lines, in diameter, of the weight of thirty-ſeven decigrams, or ſixty-ſix grains, the cube decimeter, was eaſily bended over the chimney, that is to ſay, at eight centimeters, or two inches, &c. from the upper extremity of the flame. Tin, placed in a ſmall crucible of kaolin, upon a ſupport of the ſame height, flowed in leſs than ſeven or eight minutes. Five grams, or eighty grains, of antimony in ſmall fragments, afforded in a ſimilar crucible, at the end of an hour, a degree of fuſion ſufficiently advanced to round the lower part of the button, and it is known that the loweſt eſtimation of the heat required to fuſe this metal is 431 degrees.

Tube of glaſs.

"The conſumption of oil was determined by ſeveral trials, one of which was continued for ſeven hours: It varied very little; the mean term was 34,648 grams per hour. ($1\frac{1}{4}$ oz. avoird.)

Conſumption of oil.

"With regard to the operative means by which theſe effects are obtained, we ſhall remark, firſt, the diſpoſition of the chimney, which can be raiſed or lowered at pleaſure by turning the cylinder

Mechanical conſtruction.

cylinder which ſupports it on its ſcrew, which has nine threads; by which means the true ſituation to produce the beſt effect is obtained, whatever may be the difference of ſize between one glaſs and another. The ſcrews are pewter, but the reporters obſerve, that it may be better to make one of them iron.

" The advantages of raiſing the oil by a pump moved by a ſpring are real, and prevent the wick from becoming charred for want of a conſtant ſupply of oil; beſides which, as the flame is kept at a diſtance from the ſocket, it can neither calcine the metal nor depoſit that cruſt of hardened oil which ſo ſoon alters the effects of other lamps.

The Commiſſaries expreſs their approbation of the expedient by which the oil is prevented from leaking through and arriving at the mechaniſm, and ſtate, that they examined the mechaniſm, which they find to be well calculated to produce the deſired effect, and to prove as durable as could be wiſhed.

Illumination judged by reading a book.

The brilliancy of effect is ſpoken of in high terms of approbation. They found that when a white gauze ſhade was put over the lamp, they could read in the *Anacharſis* of *Didot* at the diſtance of 78 decimeters, or about 24 feet; and that they could read in the ſame book at the diſtance of 89 decimeters with the flame uncovered. They do not mention the type of the book.

Blue chimney.

With a chimney of blue glaſs they could read at 72 decimeters.

When a ground glaſs was placed before the flame, they could read at the diſtance of 75 decimeters. A candle placed behind the ſame glaſs, gave a light by which it was difficult to read at 23 decimeters.

Particular deſcription of the conſtruction of the lamp.

In addition to the report of the Commiſſion of the Inſtitute, the Editor of Les Annales des Arts gives a more minute deſcription, with an accurate engraving, which is preſented to the reader in plate IX.

Fig. 1. View of the under part of the movement of the lamp: *aa* the lower plate of braſs, which, with the ſuperior plate connected by four braſs pillars, forms the frame of the movement: *b*, ratchet wheel for winding or ſetting up the ſpring: It is pinned upon the ſquare of the barrel arbor, and is kept in its place by the click and back ſpring *c*: The two holes in the face of the wheel are intended to receive the points of a key for winding it up: *d*, a plate which receives the

the pivots: *e*, large wheel, the pinion of which is driven by the wheel of the barrel: *ffff*, ends of the pillars: *h*, the fly and its endlefs fcrew.

Particular defcription of the conftruction of the lamp.

Fig. 2. Plan of the upper fide of the movement: *i*, upper brafs plate: *k*, the bottom of the refervoir of tin: *l l l*, connecting pillars: *m m*, a circle with its fcrews, which ferves to fupport the pump and its apparatus: *n*, body of the double force pump: *o*, aperture through which the oil rifes through the afcending pipe: The elevation of this pipe is feen at *o*, fig. 3: *p*, the double arm of the piftons of the pump: *q q*, pump rods.

Fig. 3. Lateral elevation of the mechanifm: *s s*, the plate which covers the body of the lamp, fixed by four fcrews, which at the fame time connect the lower plate *u*: This plate is raifed up in the drawing inftead of being fcrewed down, in order to give a clearer idea of the connection. The middle piece *t* is perforated by two cylindrical holes, in which the piftons move which drive the oil. The piece *t* is alfo perforated above and beneath, in order to admit of two conical valves opening upwards into each cavity or pump barrel. The lower valve in each fuffers the oil to rife into the barrel as the pifton is withdrawn, and the upper fuffers it to pafs upwards when the forcing ftroke takes place. The pieces *s*, *t*, and *u*, are fecured by a piece of leather put between them before they are fcrewed together.

The dotted line *v v* denotes part of the refervoir of oil: *w*, part of the lower plate *m*: *x*, one of the pillars: *y*, the barrel containing the fpring: *z*, pinion of the large wheel: A, pinion of the wheel of the endlefs fcrew: B, the wheel which drives a crank that works the pumps: C, the leading piece which acts on the lever: D, pinion of the wheel B. E, middle wheel, taking in the pinion D, and moved by its own pinion, which takes in the great wheel *e*: F, lever that works the arms of the pump.

Fig. 4. Elevation of the natural fize of the glafs globe *r*: 1, A pivot of fteel which paffes through the glafs globe to communicate with the arms of the pump. The lower part of this pivot is fquare, in order to receive the lever of ofcillation; the upper part is round, and has the arms of the pump driven tight upon it. 2, A brafs focket in which the pivot 1, 3 turns, and is prevented from defcending by a fhoulder. The glafs

Particular description of the construction of the lamp.

globe, or pearl-ſhaped veſſel, is blown from a piece of barometer tube, and is connected in the ſocket by ſealing wax (or lac), which is inſoluble in oil. Citizen Carcel found a great difficulty in preventing the leakage of the oil to the works, which he has obviated by filling the glaſs veſſel *r* with molaſſes thickened by heat. This fluid is not acted on by oil, and anſwers the purpoſe very well, without impeding the motion.

Fig. 5. Elevation of a chandelier to which the mechaniſm is adjuſted. G, the foot containing the movement: H, reſervoir containing oil, and the pump: I, the ſtem of the chandelier, forming part of the reſervoir, and traverſed by the tube of aſcenſion *o*, which ſupplies the wick with oil: K, the apparatus of the lamp, having apertures through which the air enters. Fig. 6. Another more elegant figure propoſed by C. O'Reilly.

The number of the wheels are as follow: Barrel wheel, 108 teeth: It drives a pinion of 12 on the arbor of *e*, which has 84 teeth. The middle wheel E has 96 teeth, and a pinion of 12. The crank wheel has 90 teeth and a pinion of 8. The laſt or endleſs ſcrew wheel has 20 teeth, and a pinion of 12. And the ſcrew has two threads.

The movement will go twelve hours without winding up.

The uſe of the lamp is ſimple and eaſy. The wheel-work is either locked or ſet at liberty at pleaſure by a ſtop. In the regular proceſs, the wick is to be firſt trimmed; then the mechaniſm is to be wound up; and in the next place, the oil is to be poured in. When the ſtop is diſengaged, the oil is ſeen to riſe up to the wick, and this is the proper time to light it. When it is no longer wanted, the movement is to be ſtopped of courſe, at the ſame time that the lamp is extinguiſhed.

XI.

Note upon a peculiar vegetable Principle contained in Coffee. By RICHARD CHENEVIX, *Eſq. F. R. S. M. R. I. A. From the Author. May* 24, 1802.

Coffee heated in water, then filtered and evaporated,

IN a veſſel calculated to confine the vapour of water, I heated a conſiderable portion of that liquid upon about a pound of raw coffee imported directly from Martinico, and of

of the quality of which I was well assured. I then filtered the liquor, and reduced it nearly to dryness, in a glass evaporating dish, at a gentle heat. By this means I obtained a small quantity of a clear yellow residuum, like the most transparent horn, and of the consistence of honey. This residuum did not deliquesce, or seem to be subject to change, by exposure to the atmosphere. It was soluble in alcohol. It did not manifest either acid or alkaline properties. By some experiments I perceived it to be a substance differing essentially from all the vegetable principles with which I was acquainted; and, finding that I could obtain it pure by a method which Proust used to procure tannin, I proceeded in the following manner:

gave a clear yellow residue,

essentially different from other vegetable products.

I poured a solution of muriate of tin into some water which had been made to boil upon coffee, and obtained a precipitate, which I collected upon a filter, and washed. I then put it into water, and caused a current of sulphurated hidrogen gas to pass slowly through the liquor. By this process the oxide of tin combined with the sulphurated hidrogen gas, and the substance originally contained in the coffee, but which, as I shall immediately shew, had combined with the metallic oxide, was disengaged, and remained in the liquor, while the hidrogenized sulphuret of tin was precipitated. It then remained only to evaporate the liquor to obtain the vegetable principle. In this state it exhibited nearly the same appearance as before it had been combined with the oxide of tin, but seemed of a lighter colour, and more clear and transparent, being freed, as I suppose, from all extractive or other matter.

It was precipitated by muriate of tin;

and the tin thrown down by sulph. hidrogen.

The disengaged new vegetable principle was thus obtained purer.

Imagining it now to be sufficiently pure, I dissolved it in a very small proportion of water, and examined it chemically.

Its habitudes. Soluble in alcohol, and in water: bitter taste.

The solution was of a bright horn colour; had a bitter taste, though not unpleasant. It was neither acid nor alkaline.

Solution of potash, of soda, or of ammonia, poured into the liquor, changed its colour to a bright garnet red.

Alkalies turn it red;

Nitric acid produced a similar effect.

as did nitric acid.

Very concentrate solutions of the alkaline carbonates did not cause a precipitate, as in a solution of tannin.

No precipitate by alkaline carbonates.

Sulphuric acid became of a dirty brown colour with the solution, but no other change was apparent.

Sulphuric acid, little change.

With muriatic, phosphoric, and the vegetable acids, there was no change but what would naturally result from a mixture of the colours of both liquors.

Other acids, none.

Solution of iron; beautiful green; precipitate.

With any ſolution of iron in which there was not exceſs of acid, the liquor paſſed to a beautiful green; and, if it was concentrate, there was a green precipitate. Salts formed of the red oxide of iron ſucceeded the beſt; and the reciprocal action of this principle and iron is almoſt as delicate as that of either gallic acid or tannin and iron.

Muriate of tin, yellowiſh precip.

With muriate of tin there was a very abundant yellowiſh precipitate, which was a combination of the new vegetable principle with oxide of tin. Both this precipitate and that with iron are ſoluble in all the acids, and the liquors loſe their colour.

Waters of lime, barytes, ſtrontian.

Lime-water did not cauſe any precipitate in this liquor, nor did ſtrontia-water. Barytes-water gave a fawn coloured precipitate. With lime-water tannin gave a bluiſh-green precipitate; and nearly the ſame with ſtrontia-water, as alſo with barytes-water.

Gelatine gave no precipitate.

A ſolution of gelatine did not give any precipitate with this vegetable principle. The effect of tannin upon gelatine is well known.

Hence this principle is different from any other.

By theſe experiments it is proved that the principle bears ſufficient characters to diſtinguiſh it from tannin, or any other vegetable principle with which we are acquainted. The only property which it poſſeſſes in common with tannin, is its affinity for oxide of tin, while it is clearly diſtinct from tannin in every other point.

Coffee does not exhibit tannin till roaſted.

It is evident that coffee, before it is roaſted, does not contain tannin. A ſolution of gelatin poured into a decoction of well roaſted coffee, gives, however, an immediate precipitate; and the precipitate is the combination of tannin with gelatin. Meſſrs. Prouſt, Seguin, and Davy, have obſerved, that heat developes the tannin principle in many vegetables. In a commercial point of view, it might be advantageous to examine, whether thoſe vegetables are not ſuch as, before they are heated, contain this new principle. Although I did not perceive that the principle, when inſulated from the entire vegetable, was converted by heat into tannin; yet the preſence of the other component parts of the vegetable may influence the diſtribution of elements in ſuch a manner as to produce combinations different from what the ſeparate principles will afford. I have not had an opportunity of extending theſe reſearches any further in the vegetable kingdom.

Whether this new principle, *while in the vegetable*, may not be converted into tannin by heat.

XII. *Account*

XII.

Account of some Experiments performed upon a Scale of considerable Magnitude, and principally by the Agency of Frost, to produce Sulphate of Soda, Carbonate of Magnesia, and Muriate of Ammonia, from Sulphate of Magnesia, Carbonate of Ammonia, and Muriate of Soda. By H. CAMPBELL, *M. D. From the Author.*

THE selected saline substances were, Epsom salt, or the sulphate of magnesia; table salt, or the muriate of soda; and concrete hartshorn drops, or ammoniacal carbonate. Of the proper mixtures, calculation was formed upon a supposed acquaintance with the table of agents entering into the composition of each body.

Sulphate of magnesia and mur. soda gave *sulph. soda* and mur. mag. To the latter was added carbonate of ammonia, and the products were, *muriate of ammonia* and *carb. of magnesia.*

TABLE.

	Acid.	Alkaline Earth.	Water.
Sulphate of magnesia,	33 sulphuric	19 magn.	48
Muriate of soda,	52 muriatic	42 —	6
Ammoniacal carbonate	45 carb. acid	43 —	12

The expected and unavoidable products were,

	Acid.	Alkali.	Water.
Glauber salts, or sulphate of soda, containing - - - -	27 sulph.	15 magn.	58
Salited or muriated magnesia, containing - - - -	34 mur.	41 —	25

And upon the addition of the ammoniacal carbonate to the salited magnesia,

Carbonate of magnesia - -	30 carbon	48 earth	22
Muriate of ammoniac - -	52 —	40 —	8

The salited magnesia must only be considered as an intermediate product.

To avoid the detail of various experiments, those found to be most practicable will be only related, connected with the most practicable apparatus.

Detail of the experiment.

In iron pans of 150 gallons each, due proportions of sulphate of magnesia and muriate of soda were put in water in proportion to the laws of solubility and crystallization. The saline liquor was pumped into troughs lined with tinned iron plate. The troughs were thirty feet long, one foot wide, and three inches

inches deep. Some of the liquor was expofed in butcher's trays made of afh, four feet long, two feet wide, and a few inches deep. Thermometer, Fahrenheit, 26 ——: Product, Glauber falt, or fulphate of foda, and muriate of magnefia, in quantity correfponding with the proportion of the requifites in the mixture.

Obfervations.

Contrary to my ufual practice, the faline liquor in this experiment was put out in a cold ftate: The confequence was, a precipitation of the fulphate of foda in the form and appearance of fl. benzoin. The whole of the Glauber falt was thrown down; the mother liquor containing only muriate of magnefia. While the mother liquor contains Glauber falt, it will feel and appear in fome meafure harfh and faline; but when totally deprived of Glauber falt, new appearances will take place. The tinned iron gutters, inftead of fhewing a difpofition to ruft, will be remarkably clean and brilliant; the mother liquor foft and oily to the touch, and will pour fmooth. To obtain the Glauber falt in diftinct cryftals, the product of this experiment was rediffolved in a leaden evaporator, in its own water of cryftallization, and pure water added, enough to make a light faline liquor. This liquor was put into leaden coolers ten feet long, five feet wide, two inches deep, ftanding in a room, therm. 57, doors and windows were left open. In the morning I found and took about three or four large bafkets of Glauber falts, perhaps the moft perfect cryftals ever feen; cryftals from nine to fifteen inches long, and one inch broad.

Very fine cryftals of Glauber's falt.

Cryftallization by froft without fecond folution.

To avoid the trouble and expence of diffolving the fulphate of foda twice, while the froft continued the operations were continued in the large way, in the proportions of fulphate magnefia, muriate foda, and water, as mentioned, with the precaution of pumping the liquor from the pans into the troughs and trays, HOT, but beneath the boiling point. The whole of the Glauber falt was always formed when the froft was fevere, as at thermom. 23, in beautiful and diftinct cryftals, half an inch broad and four or five inches long.

PRECAUTIONS AND REMARKS.

Various cautions, &c. on the management of falts.

Caft iron pans, heated in frofty weather, frequently burft. Boilers fhould be made of fheet iron. Jointed wooden veffels will not hold hot faline liquors. In melting falts different fteps muft be adopted. Salt holding little water of cryftallization,

as muriate of ſoda, ſhould be added to a ſufficient quantity of water; without a ſufficient quantity of water in a heated pan it will fuſe into a maſs; ſurface being loſt, the difficulty of ſolution is almoſt inconceivable.

Various cautions, &c. on the management of ſalts.

Salts holding an abundance of water of cryſtallization, as ſulphate of magneſia aud ſulphate of ſoda, ſhould be diſſolved by a gentle heat in their water of cryſtallization, and what further water may be required, ſhould be then added. Salts abounding in water of cryſtallization, added to warm water, will conglomerate, the caloric is abſorbed, and although a ſmall proportion of the ſalt is diſſolved, the remainder is made colder: it is governed by the ſame law as operates on the ſolution of ice. The power of adding water evades a ſuperabundant quantity of mother liquor, and enſures a quantity of cryſtals.

To obtain the ſaline liquor white and pellucid, it ſhould not remain long in the iron pans, not more than two hours.

The firſt liquor pumped from the pans ſhould be equally mixed with the laſt liquor, to obtain regular cryſtals in conſiderable quantity. It is known that the under layers of ſaline liquors are heavieſt.

Very light liquors afford the largeſt ſhoots or cryſtals. Very large cryſtals appear opake, and are never of a good colour; hence the neceſſity of breaking down to make many ſurfaces, to be acted upon by light.

Heavy ſaline liquors, holding Glauber ſalts, produce a quantity of ſalt in a compound ſtate of cryſtallization.

A ſaline liquor, of a proper weight, will give numerous cryſtals, long and narrow, and theſe will be found in ſhape and colour moſt perfect. Such cryſtals were formed by the power of froſt in the troughs and trays, acting upon *hot* ſaline liquor, in the *night*.

The *darkeſt* nights produced the beſt cryſtals. Perhaps the night was colder than the day. At any rate, the maxim of the neceſſity of light towards cryſtallization is doubtful in this inſtance. Light is not neceſſary to the cryſtallization of ſulphate of ſoda.

Light not neceſſary to the cryſtallization of ſalts.

The neceſſity of air towards cryſtallization, as ſhewn by the uſual experiment of admitting air to a ſolution of ſalt in a veſſel which did not previouſly contain air, is frequently exemplified in the large way. Salts ſometimes will not ſhoot in a liquor

Air is neceſſary.

covered

covered by a ſtrong pellicle. The workmen break the pellicle, and the ſalts inſtantly ſhoot. Cryſtallization, under ſuch circumſtances, is often *immediately excited* by gently running a ſaline liquor from one gutter into another gutter.

Electricity is of conſequence.

Saline liquors, under an improper atmoſphere, will not form regular cryſtals, but accrete into ſhapeleſs lumps. The electric and non electric ſtates of the atmoſphere govern cryſtallization. Many a maker of Glauber ſalts becomes fatigued from not underſtanding the latter circumſtance.

Effect of froſt in theſe experiments.

From what has been ſaid, the young chemiſt muſt not ſuppoſe that the elective attractions are altered, and the new compounds in theſe experiments created by means of froſt. From ſulphate of magneſia and muriate of ſoda, ſulphate of ſoda and muriate of magneſia can be obtained without froſt. Froſt only poſſeſſes a readier power of converting, from what was formed in a large portion of fluid, Glauber ſalt or ſulphate of ſoda, ſpeedily, from a hot liquid into a cold fluid ſtate. The apparent cauſe conſiſts in the power of ſevere froſt in ſuddenly diſplacing the caloric, which becomes a diſaggregative power.

H. CAMPBELL.

21, *Fleet Street, May* 20, 1802.

XIII.

Note of Citizen VAUQUELIN *reſpecting the Boracite, called Magneſio-calcareous Borate by the French Chemiſts.*

Component parts of the boracite.

THIS foſſil, of which the properties have afforded a number of intereſting obſervations to philoſophers and mineralogiſts, has been analyzed for the firſt time by M. Weſtromb, who found it to contain,

Boracic acid	68
Magneſia	13,05
Lime	11
Alumine	1
Oxide of iron	0,75
Silex	2
	95,80

Citizen

Citizen Vauquelin having examined this ſubſtance ſome time ago with Mr. Smith, who brought a conſiderable quantity, thought himſelf juſtified in concluding, that the lime is no eſſential part in its compoſition, becauſe its powder effervesces with the acids, and the ſmall quantity of lime which chemiſts find in their analyſis, did not appear to exceed that which the degree of effervescence has ſince indicated. They then attempted, by weak acids diluted with much water, particularly by the acetous acid, to ſeparate the portion of carbonate mixed with the borate; but they did not ſucceed, becauſe the acetous acid, even though feebly, likewiſe attacked the borate. They then left the queſtion undecided, for want of tranſparent cryſtals which did not effervesce with the acids.

But ſince that period, Mr. Stromager having ſupplied Citizen Vauquelin with cryſtals of this deſcription, perfectly tranſparent, he ſubjected them to new experiments, with the intention merely of aſcertaining the preſence of lime.

He mixed their powder with muriatic acid, and when the ſolution was effected by means of a gentle heat, he evaporated to dryneſs in order to expel the exceſs of acid, and afterwards diſſolved it in a ſmall quantity of cold diſtilled water. By this method he ſeparated moſt of the boracic acid, which was in very white brilliant plates. He diluted the ſolution with water, and mixed a certain quantity of oxalate of ammonia, which, as chemiſts know, is the beſt re-agent to ſhew the preſence of the ſmalleſt quantity of lime contained in any fluid, provided it contain no exceſs of acid. Nevertheleſs, it exhibited no ſign by which the exiſtence of that ſubſtance could be ſuſpected.

Experiments which ſhew that lime is not an eſſential part of this mineral.

In order to aſcertain that the ſmall quantity of boracic acid diſſolved by water, at the ſame time as the muriate of magneſia, did not oppoſe the precipitation of the lime, he mixed a portion of the muriate of lime, which did not certainly amount to a fiftieth part of the borate employed, and a cloud was immediately produced through the whole of the fluid.

On the other part, he decompoſed the artificial borate of lime in the ſame manner as the natural borate, and obtained, by the addition of oxalate of ammonia, a very abundant precipitate.

It is therefore evident, that if the natural borate had contained only one hundredth part of its weight of lime, it would have given ſome indication by the methods employed by C. Vauquelin

quelin. He therefore concludes, that the natural magnesian borate, when perfectly transparent, does not contain lime, and that that which is found in opake crystals is interposed in the state of carbonate, and causes their opacity.

This substance must not therefore any longer be considered as a triple salt, under the name of magnesio-calcareous borate, but simply that of magnesian borate.

Bulletin des Sciences, No. 60.

XIV.

Facts and Observations tending to explain the curious Phenomenon of Ventriloquism. By Mr. JOHN GOUGH *.

The power of distinguishing the character of sound has been long considered; but not of its direction.

IN the excellent Paper from which I have extracted the following pages, the author begins by observing, that the power of the ear to distinguish very slight variations of tone has long been a subject of universal notice; but that anothor very remarkable power, namely that of ascertaining the direction of sound, remains still without explanation. We perceive not only the tone, intensity, and character of sound; but also whether it arrives from the right or the left, from above or below, and this with a degree of precision which is of great and eminent utility in the concerns of life.

All sounds arrive in the ear from the same direction.

To investigate the foundation of this habitual judgment, we cannot have recourse to analogy of the sense of hearing with that of vision. The last direction of the ray of light is physically impressed by the pencil which enters the organ of that sense: but in the ear the undulations of sound are all made to strike the instrument of perception in the same direction, namely that of the auditory passage. The author, therefore, is led to attend to other facts and observations. He found by experiment, with an instrument constructed to afford the same equal sound by blows from the action of a spring, that he could judge of an increase or diminution of distance, to the one hundred and twentieth part of the whole range. He also remarks,

Our knowledge of the direction of sound does not depend on the impulse in the ear: but on other facts. Statement.

* Partly abridged and partly extracted from his "Investigation "of the Method whereby Men judge by the Ear of the position of "Sonorous Bodies relative to their own Persons." Manchester Memoirs, vol. V. part 2, page 622. London, 1802.

that

that ſound is moſt effective, when its courſe is moſt directly oppoſed towards the organ; and ſhews by experience, that the head itſelf is a ſenſitive ſolid, capable of perceiving ſounds by their action upon its ſurface. Hence when the differences of effect of the ſame ſound, from its difference of diſtance and direction with regard to the two ears, and the two ſides or portions of the head are conſidered, there will be a ſufficiently great variety in the ſenſations to mark the circumſtance of direction, and produce an habitual power of diſcrimination in this reſpect as in all others, which continually affect our daily operations.

Diagram to ſhew the effects of ſound on the head.

Direct hearing.

The annexed diagram, Fig. 2, Plate VII. is given to explain the phenomena which ariſe from the pulſes of ſound being obſtructed by the hearer's head, as they move in the horizontal plane paſſing through his ears, which caſe ought to be treated ſeparately from the more complex one that comprizes the angle of elevation, along with the horizontal diſtance from the axis of hearing. When the ſonorous object ſtands directly in front of the hearer, the ſemicircle A C B may be ſuppoſed to repreſent the horizontal ſection of his head, paſſing through the places of the ears, E and F, and the axis of hearing E F; alſo let G be the place of the ſounding body, which, according to the conditions of the caſe, lies in the plane A C B produced, and likewiſe in the right line G S, which biſects E F at right angles; ſeeing then E F is biſected by the perpendicular S G, the arch E C F is alſo biſected by the ſame in the point C. Draw L G, G K, to touch the circle in T and P, then will the arcs T E C, and C F P be equal. Now all the pulſes which do not move in right lines, contained in the angles T G S and S G P fly off without touching the circle; conſequently they add nothing to the ſound impreſſed on the ears by the body placed at G, whether the places E and F be ſuppoſed to lie in the arcs T C and C P, or without them. But the ſame number of pulſes equal in force will fall in a given time, and in ſimilar directions, on the arcs T E C and C F P as well as on the ears ſituated at E and F; and it is equally manifeſt, that the ſame reaſoning will apply to two equal and ſimilar ſolids, conſtructed upon the equal and ſimilar planes, E C S and F C S. Now ſound, though it be formed in the ears, is very much increaſed by the vibrations excited in the contiguous parts of the head by the pulſes

pulſes which fall upon them: therefore, as often as the two portions of the head, which are ſeparated by the vertical plane perpendicular to the axis of hearing, are equally agitated by the pulſes of the ſame ſound, the ears are alſo equally affected from the ſame cauſe; which never happens, as we learn from the teſtimony of the other ſenſes, unleſs the ſounding body be placed ſomewhere in the right line that biſects the axis of hearing at right angles. In this manner men are taught by experience to draw a general inference from a general obſervation; they therefore conclude a body to be ſituated directly before or behind their perſons, as often as the ſound of it ſtrikes both their ears with equal forces.

Phenomena of oblique hearing.

The phenomena of oblique hearing remain to be explained; which caſe occurs as often as the ſounding body is ſituated in the horizontal plane, but not in the right line that biſects the axis of hearing at right angles. Let M be the place of the ſounding body, and draw M O to the centre of the circle; alſo let O C biſect the arc E C F, and take O G in it equal to O M: alſo draw W M, M R, P G, G I tangents to the circle. Now ſuppoſe a ſound equal to that at M, to proceed from G, then the latter would have the ſame effect on the arc T C P that the former has on W D R, becauſe the arcs are manifeſtly equal, and alike ſituated relative to the points M and G. But the ſound proceeding from G is a caſe of direct hearing, conſequently the ears placed at E and F receive equal impreſſions from it, which is not the caſe with the pulſes that flow from M. For though the forces imparted to the two arcs, T C P, W D R are equal, they do not fall equally on the circle in reſpect to the points E and F, which repreſent the ears; the ſound therefore coming from M ſtrikes theſe organs with unequal forces, as may be eaſily inferred from the figure.

Practical delicacy of perception as to the courſe of ſound.

The judgment of the direction of ſounds by Mr. Gough's ear was preciſe to about eight degrees of poſition as to the horizon, and about ten degrees as to elevation above its plane.

Deception of hearing from echos.

The faculty by which men judge of the direction of ſounds muſt inevitably be liable to deception, whenever the ſonoriferous undulations are made to arrive in a direction which is not from the ſonorous object, as in the caſe of echo; and theſe deceptions will be the more ſtriking, as we are apt to rely on the teſtimony of the ſenſe with the moſt implicit confidence.

Upon

Upon this which conſtitutes the ſupplementary part of the preſent eſſay, I ſhall copy my author without farther abridgement or ſelection, page 644.

"Any perſon who has had occaſion to walk along a valley obſtructed with buildings, at the time that a peal of bells was ringing in it, will aſſent to the truth of the circumſtance here alluded to *. For the ſound of the bells inſtead of arriving conſtantly, at the ears of a perſon ſo ſituated, in its true direction, is frequently reflected in a ſhort time from two or three different places. Theſe deceptions are in many caſes ſo much diverſified by the ſucceſſive interpoſitions of freſh objects, that the ſteeple appears, in the hearer's judgment, to perform the part of an expert *ventriloquiſt* on a theatre, the extent of which is adapted to its own powers, and not to thoſe of the human voice.

Inſtance of the ſudden changes of direction of the ſound of bells.

"The phenomenon has often attracted my attention; and the ſimilarity of effect which connects it with ventriloquiſm, convinces me every time I hear it, that what we know to be the cauſe in one inſtance is alſo the cauſe in the other: I mean that the echo reaches the ear, while the original ſound is intercepted by *accident* in the caſe of the bells, but by *art* in the caſe of the ventriloquiſt. In order that the cauſe which gives riſe to the amuſing tricks of this uncommon talent may be pointed out with the greater clearneſs, it will be proper to deſcribe certain circumſtances that take place in the act of ſpeaking, becauſe the ſkill of the ventriloquiſt ſeems to conſiſt in a peculiar management of them. Articulation is the art of modifying the ſound of the larynx, by the aſſiſtance of the cavity of the mouth, the tongue, teeth, and lips. The different vibrations, which are excited by the joint operation of the ſeveral organs in action, paſs along the bones and cartilages, from the parts in motion to the external teguments of the head, face, neck, and cheſt; from which, a ſucceſſion of ſimilar vibrations is imparted to the contiguous air, thereby converting the ſuperior moiety of the ſpeaker's body into an extenſive ſeat of ſound, contrary to general opinion, which ſuppoſes the paſſage of the voice to be confined to the opening

This effect is ſimilar to that of ventriloquiſm.

The human voice does not iſſue from the mouth only.

* Viz. that a ſudden change of direction in ſound will be perceived when the original communication is intercepted, provided there be a ſenſible echo.---N.

4 of

of the lips: there are but few perſons, I imagine, who have not ſome time or other witneſſed an incident, which ſhews the vulgar notion to be erroneous in this particular. For if a man ſtanding in a cloſe apartment ſhould happen to apply his face to a loop hole, or narrow window, in order to ſpeak to ſome perſon in the open air, a by-ſtander in the room with him will hear his voice, not indeed in its natural tone, but as if it were ſmothered by being forced to iſſue from a hollow caſe; but the circumſtance of his words being heard diſtinctly, by one who cannot receive them from his mouth, proves the vibrations requiſite for their production to be conveyed through the ſolid parts of the ſpeaker's body, agreeably to the preceding aſſertion. The reaſon why we generally conclude the voice to be confined to the opening of the mouth, appears to be this. Thoſe pulſes which eſcape from the aperture are the ſtrongeſt, they therefore ſurpaſs the weaker vibrations of the contiguous parts; for when a number of ſounds moving in different directions ſtrikes the ear at the ſame inſtant, the hearer does not notice their ſeveral places, but refers all of them to the quarter in which the moſt powerful is perceived. For inſtance, when a man ſtands at a ſufficient diſtance from an extenſive obſtacle, his words are anſwered by an echo; but let him make a loud uninterrupted noiſe, neither he nor any body near him hears two voices whilſt his continues, but as ſoon as the noiſe ceaſes the echo is perceived. This does not happen becauſe the one begins the moment the other ends; but the reflected ſound being the weaker of the two, it is ſmothered by that which precedes it.

Inſtance.

Effect of undiſtinguiſhed echos upon the voice or an inſtrument.

"We have ſeen in what manner ſecondary or reflected ſounds are ſmothered by their principals; but though the places of ſuch ſounds are not recognized by the ear, their effects do not die away unnoticed: for the reverberated pulſes mingle with thoſe which come immediately from the ſounding body, and thereby alter the ſenſation, which, without their interference, would be leſs compounded. This is the reaſon why the ſame muſical inſtrument has one tone in a cloſe chamber, where its notes undergo a multiplicity of reverberations, and another in the open air, where the reflections are few in compariſon.

Caſe of an orator;

"But it is time to apply the preceding facts to the ſubject in hand; and it will be proper to begin with a familiar example. When an orator addreſſes an audience in a lofty and ſpacious room,

room, his voice is reflected from every point of the apartment, of which all present are made fenfible by the confufed noife that fills up every paufe in his difcourfe; neverthelefs every one knows the true place of the fpeaker, becaufe his voice is the prevailing found at the time. But were it poffible to prevent his words from reaching any one of the audience directly, what then would follow? Undoubtedly a complete cafe of ventriloquifm would be the confequence, and the perfon fo circumftanced would tranfport the orator, in his own mind, to the place of the principal echo, which would perform the part of the prevailing found at the inftant. This he would be obliged to do, becaufe the human judgment is bound, by the dictates of experience, to regard the perfon as infeparable from the voice; and the deception in queftion would be unavoidable, being produced by the fame concurrence of caufes which makes a peal of bells, fituated in a valley, feem to change place in the opinion of a traveller. It is the bufinefs of a ventriloquift to amufe his admirers with tricks refembling the foregoing delufion; and it will be readily granted, that he has a fubtle fenfe, highly corrected by experience, to manage, on which account the judgment muft be cheated as well as the ear. This can only be accomplifhed by making the pulfes, conftituting his words, ftrike the heads of his hearers, not in the right lines that join their perfons and his. He muft therefore know how to difguife the true direction of his voice, becaufe the artifice will give him an opportunity to fubftitute almoft any echo he chufes in the place of it. But the fuperior part of the human body has been already proved to form an extenfive feat of found, from every point of which the pulfes are repelled, as if they diverged from a common centre. This is the reafon why people, who fpeak in the ufual way, cannot conceal the direction of their voices, which in reality fly off towards all points at the fame inftant. The ventriloquift therefore, by fome means or other, acquires the difficult habit of contracting the field of found within the compafs of his lips, which enables him to confine the real path of his voice to narrow limits. For he, who is mafter of the art, has nothing to do but to place his mouth obliquely to the company; and to dart his words, if I may ufe the expreffion, againft an oppofing object, whence they will be reflected immediately, fo as to ftrike the ears of the audience from an unexpected

who may become a ventriloquift.

Conditions of that art.

The found muft iffue *from the mouth only,*

and it muft be *conveyed by echo.*

unexpected quarter, in consequence of which the reflector will appear to be the speaker. Nature seems to fix no bounds to this kind of deception, only care must be taken not to let the path of the direct pulses pass too near the head of the person who is to be played upon; for, if a line joining the exhibitor's mouth and the reflecting body approach one of his ears too nearly, the divergency of the pulses will make him perceive the voice itself instead of the reverberated sound.

Narrative of a ventriloquist.

"The only ventriloquist I ever attended, acted in strict conformity to the preceding theory of this curious paradox in the science of acoustics. His audience was arranged in two opposite lines, corresponding to the two sides of a long narrow room, The benches on which they were seated reached from one end of the place to the middle of it, the other part remaining unoccupied. The feats exhibited by him were the three following. *First:* he made his voice come from behind his audience, but it never seemed to proceed from any part of the wall, near the heads of the people present; on the contrary, it was always heard resembling the voice of a child, who seemed to be under the benches. He stood during the time of speaking in a stooping posture, having his mouth turned towards the place from which the sound issued; so that the line joining his lips and the reflecting object, did not approach the ears of the company. *Second:* advancing into the vacant part of the room, and turning his back to the audience, he made a variety of noises, that seemed to proceed from an open cupboard which stood directly before him, at the distance of two or three yards. *Third;* he placed an inverted glass cup on the hands of his hearers, and then imitated the cries of a child confined in it. His method of doing it was this; the upper part of the hearer's arm laid close along his side; then the part below the elbow was kept in an horizontal position with the hand turned downwards, which was done by the operator himself. After taking these preparatory steps, the man bent his body forwards in a situation which presented the profile of his face nearly to the front of his hearer, whilst his mouth pointed to the cup; in which posture he copied the voice of a confined child so completely, that three positions of the glass were easily distinguished by as many different tones, *viz.* when he pressed the mouth of the cup close against the palm, when one edge of it was elevated, and when

Voice of a child from beneath certain benches:

noises from a cupboard,

cries from an inverted cup.

Method of operating.

when the veſſel was held near the hand but did not touch it. The ſecond and third inſtances of ventriloquiſm afford ſtrong proofs, that this deluſive talent is nothing more than the art of ſubſtituting an echo for the primary ſound; for, beſides the change perceivable in the direction of the voice, it was found to be blended with a variety of ſecondary ſounds; ſuch as we know by experience are produced as often as a noiſe of any kind iſſues from a cavity. I have already made ſome remarks on this ſpecies of knowledge; but it would be improper to diſmiſs the ſubject without noticing the accuracy, with which the ear recognizes the finer modifications of ſounds, and their cauſes. I have frequently obſerved, that a certain waterfall makes a flatter and duller noiſe when the ground is covered with ſnow, than that which it affords at other ſeaſons. The human voice alſo undergoes a ſimilar change within doors, by ſtriking a multiplicity of ſoft bodies, ſuch as a number of piles of wool, or a crowded congregation in a church.

Remarks and obſervations.

The method of preventing the vibration of the vocal organs from reaching the external teguments, is ſtill wanting to complete this theory of ventriloquiſm; and I preſume it can only be ſupplied by an adept in the art. I muſt therefore diſmiſs the ſubject unfiniſhed, becauſe I have no pretenſion to that character.

Difficulty not ſolved.

XV.

An Analyſis of a Mineral Subſtance from North America, containing a Metal hitherto unknown. By CHARLES HATCHETT, *Eſq.* *.

IN the courſe of the laſt ſummer, when I was examining and arranging ſome minerals in the Britiſh Muſeum, I obſerved a ſmall ſpecimen of a dark-coloured heavy ſubſtance, which attracted my attention, on account of ſome reſemblance which it had with the Siberian chromate of iron, on which at that time I was making experiments.

Specimen of mineral obſerved in the Britiſh Muſeum.

Upon referring to Sir Hans Sloane's catalogue, I found that this ſpecimen was only deſcribed as "a very heavy black ſtone,

Hiſtorical particulars.

* Philoſ. Tranſ. 1802.

"with golden ſtreaks," which proved to be yellow mica; and it appeared, that it had been ſent, with various ſpecimens of iron ores, to Sir Hans Sloane, by Mr. Winthrop, of Maſſachuſets. The name of the mine, or place where it was found, is alſo noted in the catalogue; the writing however is ſcarcely legible: it appears to be an Indian name, (Nautneauge) but I am informed by ſeveral American gentlemen, that many of the Indian names (by which certain ſmall diſtricts, hills, &c. were forty or fifty years ago diſtinguiſhed,) are now totally forgotten, and European names have been adopted in the room of them. This may have been the caſe in the preſent inſtance; but, as the other ſpecimens ſent by Mr. Winthrop were from the mines of Maſſachuſets, there is every reaſon to believe that the mineral ſubſtance in queſtion came from one of them, although it may not now be eaſy to identify the particular mine.

§ I. DESCRIPTION OF THE ORE.

Deſcription.

The external colour is dark browniſh gray.

The internal colour is the ſame, inclining to iron gray.

The longitudinal fracture is imperfectly lamellated; and the croſs fracture ſhews a fine grain.

The luſtre is vitreous, ſlightly inclining in ſome parts to metallic luſtre.

It is moderately hard, and is very brittle.

The colour of the ſtreak or powder is dark chocolate brown.

The particles are not attracted by the magnet.

The ſpecific gravity, at temp. 65°, is 5918 *.

Experiment 1.

The ore was digeſted in muriatic acid. Little action.

Some of the ore, reduced to fine powder, was digeſted in boiling muriatic acid for about one hour.

* The following reſults of ſome experiments which I have purpoſely made, will ſhew how much the ſpecific gravity of this ore is different from that of Wolfram, and Siberian chromate of iron.

Pure Wolfram, free from extraneous ſubſtances, at tem. 65°	6955.
Siberian chromate of iron, containing ſome of the green oxide	3728.
Pure Siberian chromate of iron - - -	4355.

The Siberian chromate of iron, like all other mineral ſubſtances which are not cryſtallized, and which conſequently are not always homogeneous, muſt evidently be liable to conſiderable variations in ſpecific gravity.

The

The acid appeared to have acted but flightly upon the powder; as the former remained colourlefs, and the latter did not feem to be diminifhed. A portion, however, chiefly of iron, was found to be diffolved; for ammonia formed a yellow flocculent precipitate; pruffiate of potafh produced one which was blue; and tincture of galls, when the excefs of acid had been previoufly faturated by an alkali, formed a precipitate of a rich purplifh brown colour.

Experiment 2.

In nitric acid: little action.

Another portion of the powder was, in like manner, digefted with nitric acid; but, excepting fome flight traces of iron, this acid afforded nothing worthy of notice; the action of it upon the ore, was indeed fcarcely perceptible.

Experiment 3.

In fulphuric acid. Slight action.

Some of the pulverized ore was digefted with concentrated fulphuric acid, in a ftrongly-heated fand-bath, until nearly the whole of the acid was evaporated; the edges of the mafs then appeared blueifh, and became white, when boiling diftilled water was added.

This acid certainly acted much more powerfully than thofe which have been mentioned; but ftill only a fmall part of the ore was diffolved. It muft however be obferved, that a very copious blue precipitate was obtained by pruffiate of potafh; a plentiful purplifh brown precipitate was alfo produced by tincture of galls, after the excefs of acid had been faturated by an alkali; and, laftly, when the yellow ferruginous precipitate formed by ammonia was diffolved in diluted nitric acid, fome white flocculi remained, which were completely infoluble in the acid, even when it was added fo as to be in confiderable excefs.

From thefe experiments it was evident, that the ore could not readily be decompofed by the direct application of the mineral acids; and I therefore had recourfe to the following method, which has frequently been employed with fuccefs in fimilar cafes.

ANALYSIS.

A.

Fufion with carbonate of potafh.

A mixture of 200 grains of the powdered ore with five times the weight of carbonate of potafh, was expofed to a ftrong red

 heat,

heat, in a filver crucible. As foon as the matter began to flow, a very perceptible effervefcence took place; and, when this had fubfided, the whole was poured into a proper veffel.

The mafs, when cold, was grayifh-brown.

Solution in water left a brown refidue.

Boiling diftilled water was poured upon it; and the brown refiduum, which was confiderable, was well edulcorated upon a filter.

Precip. by nitrous acid: white flakes.

The filtrated liquor had a flight yellowifh tinge, and, being fuperfaturated with nitric acid, afforded a copious white flocculent precipitate, which fpeedily fubfided; but, although a very confiderable additional quantity of nitric acid was poured upon the precipitate, it was not re-diffolved.

The refidue did not yield to fufion by potafh.

The refiduum of the ore was dark brown, and was again melted with potafh. and treated as before; but fcarcely any effect was thus produced; the alkali was therefore wafhed off, and the powder was digefted with muriatic acid, which foon affumed the deep yellow colour ufually communicated to it by iron. After half an hour, the acid was decanted, and the refiduum was wafhed with diftilled water.

It was therefore digefted with mur. acid, which took up iron.

This refidue was fufed with potafh, and precipitated as before.

This powder was now of a much paler colour; and, being mixed with potafh, it was melted and treated as before. A confiderable precipitate was again obtained by the addition of nitric acid; and the refiduum, after being digefted with muriatic acid, was again fufed with potafh, by which means the whole was completely decompofed, after about five repetitions of each operation.

What was left was treated in the fame manner.

B.

The muriatic folution faturated with ammonia afforded *the iron.*

The muriatic folution was diluted, and, being faturated with ammonia, afforded a plentiful ochraceous precipitate; which again was diffolved in cold dilute nitric acid, and afforded a fmall quantity of a white infoluble fubftance, fimilar to that which was obtained from the alkaline folution. From this nitric folution, I then obtained, by means of ammonia, a precipitate of oxide of iron, which, being properly dried, weighed 40 grains.

C.

The nitric precipitates were a *white matter.*

The different alkaline folutions which had been made fubfequent to that which has been firft mentioned, were mixed together, and, being fuperfaturated with nitric acid, afforded the fame white infoluble precipitate; the total quantity of which, obtained from 200 grains of the ore, amounted to about 155 grains.

The

The liquor from which this precipitate had been feparated by nitric acid, was then faturated with ammonia, and, being boiled, afforded about two grains of oxide of iron. and the fluid gave a little more *iron*.

I obtained, therefore, from 200 grains of the ore,

	Grains.	Grains.
Oxide of iron - - -	42	= 197.
And of the white precipitated fubftance	155	

But, as I could not repeat the analyfis without deftroying the remaining part of the only fpecimen at prefent known of this ore, I do not wifh the above ftated proportions to be regarded as rigidly exact; it will be fufficient, therefore, to fay at prefent, that the ore is compofed of about three parts of the white matter, and rather lefs than one of iron.

§ II. PROPERTIES OF THE WHITE PRECIPITATE.

A.

It is of a pure white, and is not extremely heavy. The white precipitate

It has fcarcely any perceptible flavour, nor does it appear to be foluble in boiling water; when, however, fome of the powder is placed upon litmus paper moiftened with diftilled water, the paper in a few minutes evidently becomes red.

B.

1. When examined by the blow-pipe, it is not fufible *per fe* in a fpoon of platina, nor upon charcoal, but only becomes of a lefs brilliant white. is infufible,

2. Borax does not appear to act upon it; for the white particles are only difperfed throughout the globule. not affected by borax.

3. It produces an effervefcence when fufed with carbonate of foda, and forms a colourlefs falt; but, if too much of it be added, then the mafs, when cold, appears like a white opaque enamel. Soluble by fufion with foda,

4. When carbonate of potafh is employed, the effects are fimilar in every refpect to thofe of foda; and it may here be remarked, that the faline combinations thus formed with foda, or potafh, are foluble in water; and that thefe folutions have the fame properties as that which was formed when the ore was decompofed by an alkali. The portion of the white precipitate which may be in excefs, fubfides unaltered, when the globules are diffolved in water. and with potafh, as in the firft decompofition.

3 5. Phof-

Phofphate of ammonia forms a deep blue globule.

5. Phofphate of ammonia produces a very marked effect; for, when melted in a platina fpoon, if fome of the white fubftance be added, a confiderable effervefcence takes place, and the two fubftances rapidly unite. The globule, when cold, is deep blue, with a tinge of purple, but, when held between the eye and the light, it appears of a greenifh gray colour.

C.

It refifts nitric acid:

It is perfectly infoluble, and remains unchanged in colour, and in every other refpect, when digefted in boiling concentrated nitric acid.

D.

is foluble in fulphuric acid; which by dilution lets fall a fulphate.

It is diffolved by boiling fulphuric acid, and forms a tranfparent colourlefs folution, which is however only permanent while the acid remains in a concentrated ftate; for, if a large quantity of water be added to the folution, or if the latter be poured into a veffel of diftilled water, the whole in a few minutes affumes a milky appearance, and a white precipitate is gradually depofited, which cracks as it becomes dry upon the filter, and, from white, changes to a lavender blue colour, and again, when completely dry, to a brownifh gray. It is then infoluble in water, has not any flavour, is femi-tranfparent, and breaks with a gloffy vitreous fracture.

This fubftance is much heavier than the original white precipitate; and in a very flight degree may be diffolved by boiling muriatic acid, or by boiling lixivium of potafh.

Upon examining thefe folutions, I found that both contained the original white fubftance, together with fome fulphuric acid; fo that the precipitate obtained from the fulphuric folution by the addition of water, is a fulphate of the white matter *.

The whole is not however precipitated by water; for a part remains in folution, which may be feparated from the fulphuric acid by either of the fixed alkalis, or by ammonia.

The fulphuric folution afforded an olive precipitate by pruffiate of potafh;

The fulphuric folution is not rendered turbid by the addition of water, until fome minutes at leaft have elapfed; when, therefore,

* This fulphate is alfo precipitated when the fulphuric folution has been long expofed in an open veffel to the air; and, according as this may be moift or dry, the effect is produced fooner or later.

therefore, ſome pruſſiate of potaſh was added immediately after the water, the colour of the liquor became olive green, and a copious precipitate, of a beautiful olive colour, was gradually depoſited.

Tincture of galls, after a few minutes, cauſed the liquor to become turbid, and a very high orange-coloured precipitate was obtained. orange by tinct. galls,

A few drops of phoſphoric acid were added to a part of the concentrated ſulphuric ſolution; and, after about 12 hours, the whole became a white opaque ſtiff jelly, which was inſoluble in water. jelly by phoſphoric acid,

Potaſh, ſoda, and ammonia, whether pure or in the ſtate of carbonates, ſeparate the ſubſtance in queſtion from the ſulphuric ſolution, in the form of a white flocculent precipitate; and, when theſe alkalis are added to a conſiderable exceſs, they do not rediſſolve the precipitate, unleſs they are heated; then, indeed, the fixed alkalis act upon it, and form combinations which have already been mentioned, but which we ſhall ſoon have occaſion more particularly to notice. white flakes by alkali, alkaline combinations.

E.

1. The white precipitate, when recently ſeparated from potaſh, is ſoluble in boiling muriatic acid; and this ſolution may be conſiderably diluted with water, without any change being produced. Muriatic ſolution of the white precip.

2. A part was evaporated to dryneſs, and left a pale yellow ſubſtance, which was not ſoluble in water, and was diſſolved with great difficulty, when it was again digeſted with muriatic acid. Evap. left a yellow reſidue.

3. Pruſſiate of potaſh changed the colour of the muriatic ſolution to an olive-green; the liquor then gradually became turbid, and an olive-coloured precipitate was obtained, ſimilar to that which has been lately mentioned. But, olive precip. by pruſſiate.

4. If ſome nitric acid was previouſly added to the muriatic ſolution, then the pruſſiate changed the liquor to a graſs-green, but did not produce any precipitate. but none if nitric acid firſt added.

5. Tincture of galls, in a few minutes, formed an orange-coloured precipitate, like that which has been mentioned; but, if the acid was in too great an exceſs, it was neceſſary to add a ſmall quantity of lixivium of potaſh or ſoda, before the precipitate could be obtained. Orange pr. by galls.

White flakes by phof. acid,

6. A fmall quantity of phofphoric acid, being added to the muriatic folution, in a few hours formed a white flocculent precipitate.

and by alkalis.

7. Potafh, foda, and ammonia, alfo produced white flocculent precipitates, which were not rediffolved by an excefs of the alkalis, unlefs the liquors were heated; and, in that cafe, part was diffolved by the fixed alkalis, but not by ammonia.

Muriates of lime, magnefia, and ftrontian, gave no precip.

8. The muriatic folution did not yield any precipitate, when the muriates of lime, magnefia, and ftrontian, were added; but muriate of barytes formed a flight cloud.

Zinc threw down white flakes.

9. When a piece of zinc was immerfed in the muriatic folution, a white flocculent precipitate was obtained *.

F.

Acetous acid does not diffolve the white matter.

The acetous acid has not any apparent effect on the white precipitate, when long digefted with it.

G.

Fixed alkalis readily combine with it.

The fixed alkalis readily combine with this fubftance, both in the dry and in the humid way.

We have already feen, that the former method was employed with fuccefs in the analyfis of the ore; and the experiments made with the blow-pipe may be regarded as an additional confirmation. In each of thefe cafes, the white precipitate combined with the alkali, as foon as the heat was fufficient to caufe the latter to flow; and, when a carbonate was employed, a portion of carbonic acid was expelled.

The carbonic acid was in like manner difengaged, when the white precipitate was boiled with lixivium of carbonate of potafh, or of foda; and the folutions thus prepared, refembled in every refpect thofe which were formed by diffolving in water the falts which had been produced in the dry way.

It will be proper here to give a more particular account of thefe combinations.

Humid folution in potafh: a portion left,

1. Some of the white precipitate was digefted, during nearly one hour, with boiling lixivium of pure or cauftic potafh:

* This appears to indicate the obftinacy with which this fubftance retains a certain portion of oxigen; for we here fee that zinc does not precipitate it in the metallic ftate, but only reduces it to an infoluble oxide.

about

about one-fourth of the powder was diſſolved; and the remainder, which appeared little if at all altered, ſubſided to the bottom of the veſſel.

The clear ſolution, which contained a great exceſs of alkali, was decanted; and, by gentle evaporation, yielded a white glittering ſalt, in ſcales, very much reſembling the concrete boracic acid, affords a ſcaly glittering ſalt;

The ſalt was placed upon a filter, ſo that the lixivium might be ſeparated. It was then waſhed with a ſmall quantity of cold diſtilled water; and, being dried, remained as above deſcribed, although conſtantly expoſed to the open air. permanent in the air;

This ſalt had an acrid diſagreeable flavour, and contained a ſmall exceſs of alkali. It did not diſſolve very readily in cold water; but, when diſſolved, the ſolution was perfect and permanent. acrid taſte: ſoluble in water;

Some nitric acid was added to part of the ſolution, and immediately rendered it white and turbid. In a ſhort time, a white precipitate was collected, ſimilar to that which had been employed to neutraliſe the potaſh: and the clear ſupernatant liquor, being evaporated, only afforded nitre. precipitable by nitric acid.

Pruſſiate of potaſh was added to another portion; but did not produce any effect, until ſome muriatic acid was dropped into the liquor, which then immediately aſſumed a tinge of olive green, and ſlowly depoſited a precipitate of the ſame colour. Pruſſiate of potaſh gave no precipitate unleſs acid was added;

Tincture of galls did not affect the ſolution at firſt; but, when a few drops of muriatic acid had been added, it gradually loſt its tranſparency, and yielded an orange-coloured precipitate. nor did galls.

2. As ſo large a part of the white precipitate had remained undiſſolved in the foregoing experiment, it was digeſted again with another portion of the ſame lixivium, but without any effect. I therefore waſhed off the alkali, and boiled ſome nitric acid with the powder, until the acid was completely evaporated. After this, the powder was expoſed to a ſtrong heat in a ſand-bath. It was then again digeſted with the lixivium, and a part was diſſolved as before; but ſtill the reſiduum required to be treated with nitric acid, before the alkaline liquor could again act upon it; ſo that it was neceſſary to repeat theſe alternate operations ſeveral times, before the whole of the powder could be united with the alkali. The portion not diſſolved in potaſh—was not ſoluble till after treatment with nitric acid. It left a reſidue having the ſame properties.

3. When

Carbonate of soda, or potash, acted very nearly as pure potash.

3. When the white precipitate was digested with solution of carbonate of potash, or of soda, it was dissolved, much in the same manner as above related; and the properties of the solutions, when examined by reagents, were also similar, excepting that the orange-coloured precipitates produced by tincture of galls were of a paler colour.

Precip. by the tungstate and molybdate of potash, and cobaltate of am. Hydro-sulph. am. gave redish precip.

Tungstate of potash, molybdate of potash, and cobaltate of ammonia, being severally added to the solution of the white substance in potash, produced white flocculent precipitates.

Hydro-sulphuret of ammonia produced a reddish chocolate-coloured precipitate.

The white matter dissolved in alkali, and iron dissolved in alkali, gave, by mixture, a precipitate *of the original ore.*

4. As the ore was decomposed by being fused with potash, the following experiment affords a curious instance (among the many already known) of the change in the order of affinities produced by a difference of temperature.

Some of the solution of the white precipitate in potash, was poured into the alkaline solution of iron, which was formerly known by the name of Stahl's *Tinctura Alkalina Martis.* Potash was in excess in both of these solutions; but nevertheless a cloud was immediately produced, and a brown ferruginous precipitate was deposited.

Part of this precipitate was dissolved in muriatic acid; and the solution, being examined in the usual way, yielded a blue precipitate when prussiate of potash was added, and a purplish brown precipitate with tincture of galls.

The other part of the precipitate was digested with dilute nitric acid; which dissolved the ferruginous part, but left untouched a white flocculent matter, perfectly resembling the substance which has been so often mentioned. The precipitate therefore produced by the mixture of the two alkaline solutions, was a combination of the white matter with oxide of iron, very similar to the original ore.

(To be continued.)

XVI.

Remarks on the Mamoth. By Louis Valentine, *Physician in Chief of the Army and Hospitals of America, of several National and Foreign Societies, resident in Nancy.*

Bones of a large extinct animal found in America.

SINCE settlements have increased in North America, a great quantity of bones have been found belonging to some extinct animal, which seems to have resembled an elephant, but was

was much larger. By digging near the lakes of Canada, where the animal is called by the ſavages, the Father of Oxen; near the rivers which fall into the Ohio; towards the rivers Miami, Muſkingum, in the ſtate of Kentucky, and of Tenneſſee, &c. &c. but principally near the ſalt ſprings, pieces of ſkeletons and tuſks have been found, of an aſtoniſhing length and weight.

Enormous magnitude by admeaſurement.

We have ſeen a femur and a tibia, which, when united, muſt have been five feet and a half high; another femur, which was alone five feet long, and thirty-ſix inches in circumference in its middle or cylindrical part; ivory tuſks reſembling thoſe of an elephant, which were near ſeven feet long, and one foot ſix or eight inches in circumference at the baſe. It was not till the year 1800 that a complete ſkeleton of theſe foſſil bones could be procured. Two phyſicians of Philadelphia, Doctors Barton and Wiſtar, had in their poſſeſſion the lower jaw almoſt entire, with two teeth on either ſide, in particular, that of the former has five and three points, all quite double; but no one had the entire head.

Where moſt plentifully found.

The ſtate of New York (in the environs of the beautiful river Hudſon) has of late years been the theatre of diſcoveries of foſſil bones, a greater quantity of them having been found there than any where elſe. In 1800, by digging in the low and marſhy places of the counties of Orange and Ulſter, at three, four, and five feet deep, parts, which had never been before diſcovered, were found. Some bones, ten feet deep in the earth, were as ſound and entire as thoſe which had been met with nearer the ſurface. Some, however, were found broken, particularly thoſe of the head.

Particular fact.

In another place, eight miles from the city of New York, an upper jaw was found perforated to receive a tuſk like that of an elephant; the connection of the tuſks was by *gomphoſis;* the tuſks were evidently of ivory; the openings for the noſtrils were eight inches in diameter; and notwithſtanding that the bones of the feet afford reaſon to conclude that the animal had claws, it is ſcarcely poſſible to avoid thinking, from the ſtructure of the head, that it was a ſpecies of elephant. Some hair has even been found, three inches in length and of a dark colour, which is ſaid to have belonged to this monſtrous quadruped. Doctor Graham, a Senator, in a letter to Doctor Mitchill, Repreſentative in Congreſs, and Profeſſor of chemiſtry

Conjectures as to the ſpecies of animal.

and

and natural hiſtory at the college of Columbia, ſays on this occaſion, that it is difficult to reſolve the queſtion; "Why Providence ſhould have deſtroyed this ſpecies, which it was pleaſed to create?" Yet if it was voracious, it is happy for the human ſpecies that it has by any means become extinct.

Queſtion as to final cauſes.

We may reaſonably doubt the aſſertion of thoſe authors who affirm, that certain ſavages, Ruſſians and Greenlanders, have ſeen this animal living, and that it ſtill exiſts in the north. All that has been tranſmitted by tradition to the oldeſt Indians who have communication with the United States, and from whom information has been ſought concerning this object, is quite fabulous, and does not offer even a ſhadow of probability. Mr. Jefferſon, now Preſident of the United States, formerly paid great attention to this ſubject. (See his Account of Virginia.)

The animal has never been ſeen within human memory or tradition.

It is certain that in Siberia and in Greenland, where ſimilar foſſil bones have been found, no one has brought proof of having ſeen this animal living. We cannot ſuppoſe it to be cetaceous or amphibious, of the nature of the hippopotamus, as there are ſufficient reaſons to prove the contrary. Mr. Cuvier, whoſe reſearches into the extinct ſpecies of animals are no leſs curious than ſcientific, diſtinguiſhes the animal of Siberia, which affords foſſil ivory, from the mamoth, which latter differs principally from the former on account of its magnitude, and the points of its grinders, &c.

Conjectural remarks.

Since the time of our having received details on this ſubject (ſee the New York Medical Repoſitory, Vol. IV.) and what we have related elſewhere (ſee the third edition of Guthrie's Geography, in French, Vol. VI. page 225 and 262, publiſhed by Langlois at Paris), an account has been publiſhed in the American papers in 1801, that Mr. William Peal, proprietor of the muſeum at Philadelphia, having collected the bones found in the county of Orange, in the ſtate of New York, had ſucceeded in forming a ſkeleton, the height of which is twelve feet; the head being four feet and a half in length, and the tuſks ten feet; the other parts being in the ſame proportion.

An entire ſkeleton, twelve feet high, and its head four feet and a half long.

Almoſt all theſe bones have been found in calcareous earth. The bones of the megalonix, or great claw, of which Mr. Jefferſon has given a deſcription, were found in caverns of lime ſtone and chalk in Tenneſee. The other enormous bones of the megatherium, found in ſuch great quantities in the county of Ulſter, of which Sylvanus Miller, Eſq. has given ſome detail

Local ſituation of theſe enormous bones.

detail in a letter to Profeſſor Mitchill, are found in ſtrata of marl, which are dug to procure this calcareous ſubſtance as a manure for land. It is, nevertheleſs, ſometimes remarked, that theſe bones begin to fall in ſmall portions when, after having been from their calcareous incloſure, they are expoſed to the atmoſphere. Teeth, which were ſound and entire when extracted from the earth, become black in a ſhort time, crack, loſe their enamel, and fall into ſmall ſcales. Without ſuch a preſervative we may preſume, with our friend Mitchill, that the remains of theſe animals would have been decompoſed many centuries ago.

They ſometimes periſh by expoſure to the air.

SCIENTIFIC NEWS, &c.

Prizes of the National Inſtitute of France.

THE claſs of mathematical and phyſical ſciences having propoſed in the year 8, as the ſubject of a prize to be awarded at the public ſitting of the 15 Germinal, in the year 10, the following queſtion: *What are the characters which diſtinguiſh in vegetable and animal matters, thoſe which ſerve as ferments, from ſuch other bodies as they put into a ſtate of fermentation?* And the memoirs which have been received not anſwering the conditions of the program, the claſs propoſes the ſame ſubject again for the year 12. The prize is of the value of one kilogramme (about one hundred and twenty-five pounds ſterling). It will be given at the public meeting of the 15 Germinal, in the year 12. Memoirs muſt be ſent before the 1ſt Nivoſe of the ſame year.

Prize medal for a memoir on ferments and fermentable bodies.

Aſtronomical Prize.—Citizen Lalande has preſented the National Inſtitute of France with the ſum of ten thouſand francs (about 400 guineas) to found an annual prize, to be given by the Inſtitute to the author of ſuch diſcovery, obſervation, or work in aſtronomy, as ſhall be thought the moſt remarkable or uſeful, during the courſe of the year. The Inſtitute very highly applauded this act of generoſity in one of its members, and decreed, that thanks ſhould be conſigned in their regiſters, and commiſſaries nominated by each of the three claſſes, to preſent at the next general meeting the means of execution.

Aſtronomical prize.

Communications to the Royal Society reſpecting the Planet Ceres.

Dr. Herſchel ſent an account of the appearance of the new planet, as viewed through his teleſcopes. He had ſought for it

Extreme ſmallneſs of the planet Ceres.

it in vain, until he received Dr. Maſkelyne's determination of its place. When viewed with powers of 600 and 1200, it could not be decidedly diſtinguiſhed from a ſtar, until it was found to change its place. Its apparent diameter was not large enough to be directly determined, but it was certainly not larger than one-fourth of that of the Georgian planet, and perhaps equal only to one-ſixth. From a rough computation of its magnitude, Dr. Herſchel concludes that its real diameter is about $\frac{5}{8}$ of that of the moon: its light is of a reddiſh hue.

Mr. Gilpin alſo gave the Society an account of obſervations on the 8th and 12th of February. He found the planet's right aſcenſion change from 188° 41′ to 188° 30′, while its declination increaſed. Mr. Gilpin obſerves that its light reſembles that of the planet Mars.

Nebulous atmoſphere.

Thurſday, 25th February. A letter from Mr. Schroeter of Lilienthal, reſpecting the planet Ceres Ferdinandia, informed the Society that Mr. Schroeter had obſerved a nebuloſity round the planet, ſomewhat reſembling that of a comet: the diameter of the true diſc being 1.8″, and that of the nebula 2.6″, but the diſtinction was not always equally obſervable. Mr. Schroeter conſiders this body as of a hybrid nature, or a medium between a planet and a comet; but he imagines the apparent nebuloſity to be owing to an atmoſphere, and that, according to the different ſtates of this atmoſphere, the light reflected from the planet is either white, bluiſh, or reddiſh.

A table of obſervations of the ſame planet was alſo communicated by Mr. Mechain, through Sir Henry Englefield.—(*This article is taken from the Journal of the Royal Inſtitution.*)

Leonardo da Vinci.

The lovers of the polite arts will be pleaſed to learn, a new tranſlation of Leonardo da Vinci's treatiſe on painting (for which Pouſſin made the figures) will ſoon be ready for publication. This work has been long in the hands of Mr. Rigaud, R. A. who has paid it particular attention and care; and has given new importance and energy to the work, by arranging the chapters ſucceſſively under proper heads; by which the ſtudent will be much facilitated in underſtanding the precepts of this great maſter in the art of painting.

The reader will recollect ſome particulars of the extraordinary reſearches and very ſuperior genius of Da Vinci, by the extracts given in the former ſeries of this Journal, from an abridgement of his writings, by J. B. Venturi, quarto, II. 84.

Experiments

Experiments to prove that all Bodies, whatever may be their Nature, are obedient to the Action of Magnetism, and that this Action is sufficiently powerful to admit of being measured. By Citizen COULOMB *.

It has long ago been remarked, that platina, nickel, and several other bodies, acquire a sensible degree of magnetism; but some philosophers attribute this property only to a portion of iron not easy to be separated, and conclude, that by obtaining a greater degree of purity, we might succeed in rendering them perfectly indifferent to the action of the magnetic bar.

Magnetism of nickel, &c. supposed to be accidental.

The new experiments which Citizen Coulomb has made and repeated before the Institute, lead us on the contrary to think, that the action of magnetism extends through all nature; for none of the bodies he has yet tried was found to resist this power.

All bodies are magnetical.

But however real this action may be, it is not alike in all bodies, and in most of them it must be necessarily very small, to have escaped the attention of philosophers to this time. In order, therefore, to exhibit and to measure these results, we must begin by placing the bodies in a situation which shall allow them to yield to the weakest action.

For this purpose, Citizen Coulomb fashioned his subjects into the form of a cylinder or small bar, and in this state he suspended them to a silken thread, such as is drawn from the silk worms' cone, and in this state he placed them between the opposite poles of two magnetic bars of steel. The single thread of silk could hardly bear the weight of a quarter of an ounce without breaking, consequently it became necessary to form small bars very light and thin. Citizen Coulomb made them about seven or eight millemetres in length (or less than half an inch), with three quarters of a millemetre (or about an hundredth part of an inch) in thickness, and he gave the metals about one-third of this thickness.

Experiments made with bodies suspended to a single fibre of silk.

In his experiments he placed the steel bars in the same right line, their opposite poles being five or six millemetres farther asunder than the length of the needle intended to oscillate between them. The result of the experiment shewed, that whatever might be the substance of the needle, it always disposed

All bodies assumed the direction of two magnets which were applied.

* Communicated to the French National Institute, and inserted in the Magazin Encyclopedique, No. 22, an 10.

itſelf according to the direction of the two bars; and that if they were turned from this direction, they always recovered it, after oſcillations of which the number was often more than thirty per minute. It was therefore eaſy in every caſe to determine, from the weight and figure of the needle, the force which had produced the oſcillation.

Subjects of experiment. These experiments were ſucceſſively made with ſmall needles of gold, ſilver, copper, lead tin, ſmall cylinders of glaſs, a piece of chalk, a fragment of bone, and different kinds of wood.

Extreme delicacy of the ſuſpenſion. Citizen Coulomb has proved, in a former memoir, that the force of torſion of the ſilk thread is ſo ſlight, that in order to draw it round through the entire circle, it would require a force ſcarcely equal to the one hundred thouſandth part of a gram (or about one ſeven hundredth of a grain). A quantity ſo minute cannot therefore ſenſibly derange the meaſure of magnetic force in the different bodies, and its effect even, if it were admitted to be of perceptible magnitude, may alſo be urged in proof of the general concluſion of Citizen Coulomb, becauſe the magnetic power muſt overcome this reſiſtance of the thread in order to manifeſt itſelf. Our author gives, in the third vol. of the Memoirs of Natural Philoſophy and Mathematics of the National Inſtitute, a very ſimple formula to determine the magnetic force of a body from the time of its oſcilations, and he means to ſhew in another memoir, the method of determining this reſult in different bodies of the ſame figure placed between the poles of two bars. He thinks it now proved, that all the elements which enter into the compoſition of our globe, are ſubjected to the magnetic power, and that the whole maſs collectively forms one ſingle magnet.

Obſervations. In favour of thoſe who might be deſirous of repeating his experiments, and rendering them very ſenſible, the author remarks, that the method of ſucceeding conſiſts in diminiſhing the ſize of the oſcilating bodies. From ſome eſſays, of which the reſults terminate this memoir, it ſeems to follow, that the accelerating forces are inverſely as the maſſes are very nearly in the direct proportion of the ſurfaces; but Citizen Coulomb gives this rule only as a firſt deduction, which requires to be confirmed.

ERRATA.

In Mr. Cruickſhank's paper, p. 43, end of the firſt paragraph, for *faults* read *facts*; and p. 46, l. 26, for *computed* read *compelled*.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1802.

ARTICLE I.

Composition of Writing Ink, possessing the permanent Colour, and other essential Properties, of the Ink used for Printing. In Letter from Mr. William Close.

To Mr. NICHOLSON.

SIR,

Dalton, May 25, 1802.

Preliminary address.

IN the second volume of the Philosophical Journal, at p. 63, the preparation of indelible ink is announced. Such an article may justly be considered a desideratum. As I have for some time directed my attention to the composition of permanent ink, I send you a short memoir on the subject, which was drawn up previous to the perusal of the above-mentioned notice, and intended for a future communication.

Permanent ink an important article for the preservation of authentic testimony in writing.

THE welfare of individuals often depends very much on the testimony of writing. An atramentous composition, possessing a permanent colour on paper, and such a degree of insolubility, after desiccation, as not to suffer any injury from exposure to humidity, or the application of such chemical preparations

parations as totally eradicate the traces of common ink, would certainly be an article of confiderable value in the material compofition of thofe writings, or legal inftruments, which are intended to evince the tranfactions of the day, and to ratify the affairs of the future.

The invention of printing probably produced fome remiffnefs about the durability of ink.

The ancients were probably more interefted in the compofition of permanently coloured ink than the moderns. When every book was in manufcript, it would frequently be requifite to tranfcribe whole volumes, fometimes merely for the purpofe of procuring a copy, at other times for the prefervation of the work itfelf; in either cafe, before the writer begun a procefs fo tedious, it would be of confequence to felect fuch materials as were the moft durable, in order to protract the like neceffity, and alfo for the purpofe of enhancing the value of his labour. But fince the invention of printing, the neceffity of tranfcribing has been entirely fuperfeded; the manufcripts of modern authors after publication, have been reduced to little more value than objects of curiofity; and writers have been contented with fuch materials as are the cheapeft and eafieft to procure.

Ink with vegetable infufions and fulphate of iron,

not durable.

For a great number of years, the ink moft generally ufed by European writers has been the infufion of galls, and other aftringent vegetables containing gallic acid, rendered black by fulphate of iron, and thickened by the addition of a little gum or fugar. This compofition is well adapted to the common purpofes of writing and the difpatch of bufinefs, but its colour is liable to fade: a compofition of a more permanent colour is therefore defirable, for writings which are required to retain their primitive fignatures, and fuch as cannot be printed. Indeed, fince the difcovery of the method of totally difcharging the traces of common ink by the application of the oxigenated muriatic acid, more ferious confequences are to be apprehended from the univerfal ufe of the common atramentous fluid, than the decay of its colour from age; for it is well known, that while the fulphate of iron remains on the paper, the colour of the writing may be reftored, by wafhing the manufcript with frefh infufion of galls.

The colour may be reftored while the fulphate of iron remains on the paper.

The moft ferious objection to its ufe is, that it may be totally difcharged.

Means of preventing this by the addition of pigments.

In the quarto feries of the Philofophical Journal (Vol. IV. p. 479), there are feveral ufeful receipts for compofing ink capable of refifting the oxigenated muriatic acid. The moft material difference, however, of the compofitions there re-

 commended

commended from thofe in common ufe, confifts in the addition of pigments producing an unchangeable colour upon paper.

Ink of a permanent colour may be eafily obtained, by fufpending various pigments in an aqueous fluid, by the intervention of gum, without the affiftance of the common ingredients; but fuch compofitions are liable to one of the greateft inconveniences, for the whole of the writing may be detached from the paper by wafhing the manufcript with water. Such ink, however, was frequently ufed by the ancients.

Ancient method of compofing ink.

As a permanent colour is certainly a valuable requifite, it appears very promifing in fpeculation for the improvement of this indifpenfible article, according to the fimpleft, and perhaps the moft ancient method of compofition, to fubftitute, in place of the common mucilaginous fluid, as a compound vehicle for the diftribution and protection of the colouring matter, the folution of fome gum, or refinous fubftance, which can be diffolved in only a few liquids. After the diffipation of the thinner part of an atramentous compound properly formed with fuch a folution, the colouring fubftance will be left on the paper, combined with a fufficient quantity of tenacious matter to protect it from being injured by friction, or from being difcharged by the application of any fluid to which the writing may be expofed, without injuring the paper.

Speculation for the improvement of this kind of ink.

Many of the more volatile kinds of oils may be ufed in writing, if reduced to a proper confiftence by the addition of gum or refin. Tolerable ink may be made by diffolving 30 grains of common refin in 90 grains of oil of turpentine, and tempering the folution with $17\frac{1}{2}$ grains of lamp black, and $2\frac{1}{2}$ of indigo. In a dry ftate, this compofition refifts the action of water, but not of fpirit. Such, indeed, will be the cafe with every compofition in which the colour is merely fufpended in the fluid, and attached to the paper, by a fubftance of eafy folubility; the application of the article which produced the fluidity of the ink, will again penetrate and foften the dry compound. Thofe compofitions which contain a tenacious matter, foluble in a few articles only, and at an high temperature, will be the leaft exceptionable.

Ink with oil and refin.

Inconvenience to which every compofition will be fubject where the colour is mechanically fufpended.

A more infoluble kind of matter than refin fhould be ufed. Copal will diffolve in only a few liquids; it appears well adapted to the purpofe of retaining a permanent colour upon the

Copal is much fuperior to refin for the fufpenfion of the pigment;

the paper, if a vehicle can be found which will diſſolve a ſufficient quantity, and which, when the colouring matter is alſo added, ſhall be fluent enough in writing.

but is not altogether free from inconvenience.

Oil of lavender will diſſolve copal. I have made ſome experiments with theſe articles, and the combination ſucceeds ſo well, that I have not been inclined to uſe any other. The only inconvenience to be apprehended from the uſe of copal in the compoſition of ink, is, from its being ſoluble at a low temperature; but whether any other kind of tenacious matter of more difficult ſolubility, can be uſed conveniently in writing, I muſt leave to future diſcuſſion. The ſolution of a ſubſtance which could only be diſſolved in an high temperature, would certainly be the moſt proper vehicle to prevent alteration; for, after the deſiccation of the writing, it would be difficult to ſoften the ink in one part of a manuſcript, without expoſing the whole to a very injurious proceſs.

Compoſition of ink with oil of lavender, copal, and lamp black.

Ink may be compoſed of oil of lavender, copal, and lamp black, according to the following proportions of the ingredients:

Take of oil of lavender, 200 grs. copal in powder, 25 grs. lamp black from $2\frac{1}{2}$ to 3 grs. With the aſſiſtance of a gentle heat, diſſolve the copal in the oil of lavender in a ſmall glaſs phial, and then mix the lamp black with the ſolution upon a marble ſlab, or other ſmooth ſurface. Put the compoſition into the bottle, and keep it excluded from the air. After a repoſe of ſome hours, the ink muſt be well ſhaken and ſtirred with a piece of wire before it is uſed; if it be too thick, it muſt be diluted with a little oil of lavender, oil of turpentine, or alcohol. The facility of writing with this compoſition depends much on the quantity of the colouring matter. Three grains of lamp black to two-hundred and twenty-five of the ſolution of copal, producing ink of a full body of colour, and is nearly as much as the copal can protect from injury after the diſſipation of the oil of lavender. Two grains and an half of lamp black, and half a grain of indigo, produces ink of a paler colour, but which may be diſtributed upon the paper with the facility of common ink. A piece of ſponge *, or other organized

* The cuſtom of putting cotton in an inkſtand has been diſuſed, more particularly, I think, ſince an inkſtand, invented by the late worthy and ingenious Samuel More, of the Society of Arts, was made

ganized fubftance, muft be ufed in the inkftand, chiefly for the purpofe of cleaning the pen.

Red ink with red fulphuret of mercury.

Red ink may be made by tempering the folution of copal with red fulphuret of mercury inftead of lamp black. The following proportions of the ingredients produce a red ink which writes very well:

The recipe.

Take of oil of lavender 120 grs. copal in powder, 17 grs. red fulphuret of mercury, 60 grs. Diffolve the copal in the oil of lavender, and then mix the fulphuret with the folution upon a fmooth furface.

A manufcript written with thefe compofitions will not be injured by the procefs of bleaching.

Both thefe compofitions poffefs a permanent colour, and other effential properties of the ink ufed in printing. The oil of lavender being diffipated with a gentle heat, the colour is left on the paper furrounded with copal, a fubftance infoluble in water, in fpirits, in acids, or alkaline folutions. A manufcript, written with thefe compofitions, may therefore be expofed to the procefs commonly ufed for reftoring the colour of printed books, without the fmalleft injury to the writing; and in this manner all interpolations with common ink may be removed.

As

made and brought into fafhion about thirty years ago, by the celebrated Wedgwood. Thefe changes appear to be both injurious. For the ink in the cotton is kept blacker by the fufpenfion of the atramentous part; and if no more ink be prefent than perfectly to fill the cotton, the pen will always receive a fluid black ink, and may be charged at pleafure by a greater or lefs gentle preffure at the time of taking up, or difcharged by lodging the point for a moment upon the cotton. It is alfo very eafy to regulate the oxigenation by the air, fo as to increafe the blacknefs without fuffering mouldinefs to come on, by the fimple expedient of turning the cotton upfide down every day. As the fibres of the cotton prevent the fluid from circulating as ufual by the change of temperature produced from evaporation, the interior mafs may be confidered as in a clofed veffel while not in ufe.

Mr. More's fountain inkftand, at prefent fo univerfally in ufe, is certainly very inconvenient. The ink, it is true, is kept *in a clofed veffel*; but its colouring matter is at full liberty to fubfide, and the confumer is obliged to fill his pen from the muddy bottom inftead of the furface; and what is ftill worfe, the conical veffel into which the ink flows, is fubject to all the evils of evaporation and mouldinefs, fo as moft frequently to afford an adhefive and clogging fluid to the pen. W. N.

Source of apprehenfion. The oil of lavender will foften the dry compofition, but the writing cannot be eafily eradicated.

As copal is foluble in feveral of the effential oils, it may be expected that the application of thefe will do much injury to the writing. Such an expectation is well founded. If the writing be rubbed with a fmooth furface dipped in oil of lavender, much of the colouring matter will be difengaged, and diftributed more diffufely upon the paper; but, as fome of it will have penetrated the interior parts of the paper along with the copal, it will be extremely difficult to obliterate the traces of the pen without the erafement being perceptible. This is the principal fource of apprehenfion, but I know of no method of obviating the danger. The perufal of a memoir on the nature and preparation of drying oils, &c. by Mr. Sheldrake (Philof. Journal, 8vo. Vol. I. p. 259), fuggefts amber as a proper article for the compofition of ink, if enough of it can be diffolved in any fluid fufficiently thin for the purpofe of writing. The more difficulty there is in effecting the folution of any tenacious fubftance we ufe, the better, provided it can then be employed without inconvenience.

Amber a proper fubftance if it could be ufed.

I am, Sir,

Yours refpectfully,

WILLIAM CLOSE.

II.

Experiments and Obfervations on the Vegetation of Plants, which fhew that the common Opinion of the Amelioration of the Atmofphere, by Vegetation in Solar Light, is ill founded. By James Woodhouse, *M. D. Profeffor of Chemiftry in the Univerfity of Pennfylvania, &c.*

To Mr. NICHOLSON.

SIR,

Pater Nofter Row, May 27, 1802.

Introductory letter.

I INCLOSE for the Philofophical Journal, the refults of various experiments, made in Philadelphia in the year 1801, upon the feeds, leaves, &c. of a variety of plants, which feem to prove, that growing vegetables, contrary to an opinion almoft univerfally adopted, do not purify atmofpherical air; and that whenever

whenever they appear to afford oxigenous gas, it is by devouring the coal of carbonic acid gas for food, and leaving its oxigen in the form of pure air.

I have the honour to be,
Dear Sir,
With the greateſt reſpect,
Your moſt obedient,
And very humble ſervant,
JAMES WOODHOUSE.

First. *Of the Effects produced by the Germination of Seeds in atmoſpherical Air.*

On the 3d of June, twelve ſeeds of zea maiz were planted in earth, and confined over water in a glaſs veſſel, in ſeventy ounce meaſures of atmoſpherical air of the purity of 100, and often expoſed to the light of the ſun. On the 12th, the corn had vegetated, and was from two to five inches high. The air being examined at this time, by throwing up one meaſure of it, over lime water, in an eudiometer, gave $\frac{3}{100}$ parts of carbonic acid air. Another meaſure, after being freed from the fixed air, and mixed with an equal meaſure of nitrous air, produced an abſorption of $\frac{30}{100}$. On the 19th, the corn having grown conſiderably, and the air being tried again, no carbonic acid gas appeared, and the purity was the ſame as at firſt. On the 23d, the plants died, and the airs were found to conſiſt of $\frac{5}{100}$ fixed, and $\frac{95}{100}$ azotic gas. Germination of feeds in atmoſpheric air.

Similar experiments were made with the ſeeds of apium petroſelinum, lactuca ſativa, cucurbita citrullus, phaſeolus ſativus, ſiſymbrium, and raphanus ſativus, and with the ſame reſult.

The atmoſpherical air, in theſe experiments, appears to be reduced in purity, by its oxigen uniting to the coal of the cotyledons of the ſeed, or to that of ſome animal or vegetable matter contained in the earth in which the ſeeds are planted, or to that of ſome decayed portion of the living leaves. The air loſt oxigen by uniting to carbon; afterwards it became more pure; and laſtly its oxigen was totally abſorbed.

Ingenhouz, Humbold, and Thomſon, have obſerved, that ſoils have the property of abſorbing oxigen; but as it cannot be proved that any pure earth, or mixture of earths, render atmoſpherical air impure, it is certainly more philoſophical to aſcribe the impurity of the air to the formation of the carbonic acid, the baſe of which generally exiſts in all ſoils. The effect of veg. mould on air moſt probably from its carbon.

II. *Of*

II. *Of the Effects produced by the Growth of Plants in atmospherical Air.*

Growth of plants in atmof. air.

On the 27th of May, twelve plants of perficafia polygonum, two inches high, growing in earth, were confined in a glafs veffel in fifty-two ounce meafures of atmofpherical air, of the purity of 100, and often expofed to the influence of folar light. On the 4th of June, they had increafed about two inches in height. The air being examined at this time, was found to contain $\frac{1}{100}$ parts of carbonic acid gas, and to be reduced in purity to 80. Several young plants of rhaphanus fativus, latura ftramonium, phytolacca decandra, zea maiz, phafeolus fativus, fidum telephium, amaranthus hyboidus, cucurbita citrullus, firymbrium, and lactuca fativa, were alfo feparately confined in from forty to eighty ounce meafures of atmofpherical air, which was examined at various times, from one hour to thirty days, after the plants had been placed in it. Carbonic acid gas was generally formed, and whenever this circumftance happened, the purity of the air was diminifhed.

They produced carbonic acid gas, and diminifhed the purity of the air.

Growth of plants in oxigen diminifh its purity by carbonic acid.

Many of the fame kind of vegetables were alfo confined in forty ounce meafures of oxigenous gas, which had been well wafhed in lime water, and the purity of the air was very generally leffened, fixed air being generated. They turned of a white or yellow colour, and foon died, after being placed in atmofpherical air.

In confined plants the decaying parts afford carbon and form acid, which the living plant decompofes.

The fame effects are produced by the growth of plants as by the germination of feeds in common air, and by the fame caufes. If the leaves are confined a confiderable time, part of them decay, and the coal of the dead portion, uniting with the oxigen of the atmofpheric air, generates carbonic acid. This acid is decompofed by the living leaf. Its coal is abftracted, while its oxigen is left in the form of pure air.

But when the formation is quicker than the decompofition, the plant dies.

When the oxigen unites to the coal of the animal or vegetable matter of the foil in which the plants vegetate, or to the coal of the decayed parts of the leaves, and makes fixed air quicker than the living parts can decompofe it, the plants will fpeedily die.

When the foil contains but little of organized remains, the included plant will live much longer.

When a plant in perfect health, growing in a foil which contains little vegetable or animal matter, is confined in atmofpherical air, it will live a long time, without producing any change in it. Many of the vegetables which were the fubjects of thefe experiments,

experiments, did not affect the air in five days: fome diminifhed its purity in three hours; and others altered it in a moft flow and gradual manner, caufing little change in it in twenty days.

III. *Of the Effects produced by the Leaves of Plants in atmofpherical Air impregnated with Carbonic Acid Gas, and expofed to the Light of the Sun.* - - -

Leaves expofed to folar light in a mixture of atmofpheric and carbonic acid gas.

A handfull of the leaves of mimofa virgata, euphorbia picta, digitalis purpurea, franklinia altamaha, afparagus officinalis, coryllus avellana, rhus glabrum, ariftotochia fiphoe, and periploca græca, were feparately expofed feven hours to the light of the fun, in thirty-fix ounce meafures of atmofpheric air, impregnated with four ounce meafures of carbonic acid gas, from the carbonate of lime and fulphuric acid. The fixed air difappeared, and the atmofpheric air was fo much increafed in purity, as to devour two meafures of nitrous air.

The carbonic acid difappeared, and the proportion of oxigen in the mixture was augmented.

The leaves of thefe plants, kept over night in the fame air, gave carbonic acid gas in the morning; and its purity, in every inftance, was confiderably diminifhed.

In the dark the leaves produced carbonic acid gas.

The leaves of mimofa virgata and amygdalus perfica, were alfo feparately expofed nine hours to the influence of folar light, in forty ounce meafures of atmofpherical air, in which fixed air had been formed by leaving a fungus to putrefy it. The carbonic acid gas difappeared, and the purity of the atmofpherical air was increafed from 30 to 80.

Other leaves expofed to light, with the former refult.

IV. *The following Tables will fhew the Quantity and Purity of oxigenous Gas, obtained by expofing a fmall Handful of the Leaves of Plants to the Light of the Sun, in forty Ounce Meafures of Pump Water.*

Table of experiments on leaves expofed to folar light under pump water.

This water was taken from a well funk within a few yards of a neceffary, from which it was impregnated with carbonic acid gas, as appeared from an analyfis. The leaves were feparately expofed in glaffes arranged near each other, and from eight to thirteen comparative experiments were made at one time.

Leaves

Leaves of	Carbonic Acid Gas, in 100 Parts.	Oxigenous Gas in Drachm Meaſures.	Purity with one Meaſure of Nitrous Air.	Do. with two Meaſures.	Do. with three Meaſures.	State of the Thermometer.	Time when expoſed.
Alcea roſea - - -	From 8 to 9 Parts.	$19\frac{1}{2}$	122	146	96	105° to 110° of Fahrenheit.	July 2, 1802.
Zea maiz - - -		16	116	140	54		The day was very clear.
Amaranthus ſpinoſa -		15	120	140	68		
Meliſſa officinalis - -		13	120	130	50		
Hyſopus - - -		16	120	138	70		
Convolvulus purpureus -		8	110	110	0		
Malva rotundifolia - -		17	120	140	86		
Lavendula - - -		16	118	130	55		
Roſa centifolia - -		15	112	130	46		
Mirabilis dichtoma - -		16	110	130	40		
Convolvulus purpureis -	From 8 to 10 Parts.	13	110	120	40	100° to 115°.	July 3.
Anthemis nobilis - -		12	114	120	32		Day clear.
Hibiſcus Syriacus - -		12	118	130	65		
Polygonum aviculare -		18	114	130	50		
Amygdalus Perſica -		10	114	112	12		
Pyrus malus - - -		16	116	120	20		
Platanus occidentalis -		12	120	140	20		
Tilia Americana - -		10	120	138	40		

Leaves

Leaves of	Carbonic Acid Gas, in 100 Parts.	Oxigenous Gas in Drachm Measures.	Purity with one Measure of Nitrous Air.	Do. with two Measures.	Do. with three Measures.	State of the Thermometer.	Time when exposed.
Siriodendron tulipifia -	8.	14	112	120	25	105° to 110°.	July 4, 1801. Day generally clear.
Populus dilatata - -		14	110	132	60		
Æsculus pavia - -		13	110	130	60		
Apium petrofelinum -		12	115	132	55		
Convolvulus purpureus -		5	120	120	30		
Helianthus annuus - -		13	112	132	62		
Ruta graveolens - -		10	120	130	40		
Trifolium paluftri - -		13	120	140	55		
Datura ftrammonium -		14	112	130	80		
Hyfopus - - -		7	112	132	65		
Blattari verbafcum - -	8 & 9.	12	112	130	45	95°.	July 5. Day clear and cloudy. Twelve ounce meafures of this oxigenous air, after being wafhed in lime water, to free it from the carbonic acid gas, being expofed to a mixture of iron filings and fulphur, were found to confift of eight ounce meafures of oxigenous, and four of azotic gas.
Chelidonium majus - -		18	112	136	80		
Chryfanthimum Indicum -		14	120	142	63		
Acer glaucum - -		14	120	139	63		
Phytolacca decandra -		14	120	140	80		
Antirrhinum linaria - -		18	120	140	65		
Arctum cappa - -		12	120	140	53		
Syringa vulgaris - -		8	120	132	40		
Helianthus altiffimus -		12	120	140	55		
Polygonum Perficana -		12	120	140	80		
Cercis Canadenfis - -		12	120	140	60		
Sonicera caprifolium -		12	120	140	60		
Diofpyros Virginiana -	8 to 10.	10	120	120	30	90° to 110°.	July 6. Day clear and cloudy. Thefe leaves were gathered in the evening, and kept until morning, in a cool place.
Franklinia altamaha -		10	120	102	0		
Chionanthus Virginica -		8	120	100	0		
Arundo gigantia - -		10	120	130	32		
Afclepias Syriaca - -		9	120	80	0		
Annona triloba - -		10	120	130	40		
Magnolia glauca - -		10	110	102	0		
——— tripetala - -		16	116	130	40		
Xanthoriza tinctoria -		8	120	130	50		
Conferva civularis - -		10	120	120	30		
Alcea rofea - - -		5	110	70	0		
Sophora indica - -		7	110	80	0		
Laurus faffafras - -		10	120	92	0		

We

Opinion that plants supply oxigen to the atmosphere,

We are indebted to Dr. Prieſtley for the diſcovery, that plants expoſed to light yield oxigenous air; and ever ſince it has been made, an opinion has been adopted, that growing vegetables ſupply the oxigenous portion of atmoſpherical air, of which there is a conſtant conſumption, by combuſtion, fermentation, reſpiration, and the calcination of metals.

ill founded;

If this ſubject is attentively examined, it will be found that plants have no effect in rendering the air of the atmoſphere pure.

becauſe they afford none unleſs carbonic acid be preſent. Experiments of Prieſtley in proof;

Firſt. Whenever oxigenous gas has been obtained from vegetables, carbonic acid gas has been preſent.

Dr. Prieſtley expoſed plants to atmoſpheric air, in which ſpirit of wine and wax and tallow candles had burned out; to air which had been vitiated by the death or putrefaction of mice and fiſhes, and to air which had been frequently taken into his lungs. He alſo obſerved, that there was a ſlower and leſs production of air from rain and diſtilled, than from pump and ſtagnant water.

and of the author; tabulated.

The difference between the quantity and quality of the gas, obtained from river water and the ſame water impregnated with carbonic acid, by expoſing plants in it to the influence of ſolar light, will be ſeen by the following table:

Leaves

Leaves of	Carbonic Acid Gas, in 100 Parts.	Quantity of Gas in Drachm Meaſures.	Purity with one Meaſure of Nitrous Air.	Do. with two Meaſures.	Do. with three Meaſures.	State of the Thermometer.	Time when expoſed.
Siriodendron tulipifera -	None.	From half a Drachm to one Drachm.	55			110°.	July 7, 1801. Day very clear. The leaves were expoſed in the water of the river Schuyltril.
Cercis Canadenſis - -			70				
Tilia Americana - -			50				
Salix Babylonica - -			32				
Polygonum Perſicaria -			30				
Phytolacca decandra -			94				
Platanus occidentalis -			90				
Alcea roſea - - -			84				
Helianthus annuus - -			83				
Amygdalus Perſica - -			82				
Conferva fontinalis - -			80				
Zea maiz - - -			75				
Acer glaucum - -			90				
Seriodendra tulipifiera -	In ſome of the veſſels none, in others, from 5 to 10 Parts.	6	120	130	40	110°.	July 8, 1801. Day a little hazy, although the ſun ſhone conſtantly. The leaves of the ſame plants, in the ſame river water, impregnated with four quarts of the water, ſaturated with carbonic acid gas, from carbonate of lime and the ſulphuric acid.
Cercis Canadenſis - -		6	116	124	30		
Tilia Americana - -		5	110	160	0		
Salix Babylonica - -		5	120	100	0		
Polygonum Perſicaria -		10	120	140	70		
Phytolacca decandra -		6	120	140	42		
Platanus occidentalis -		3	110	60	0		
Alcea roſea - - -		6	120	132	40		
Helianthus annuus - -		10	120	110	50		
Amygdalus Perſica - -		6	120	138	40		
Conferva fontinalis - -		4	120	134	50		
Zea maiz - - -		4	115	125	20		
Acer glaucum - -		6	120	140	50		

It appears from this table, that the leaves of thirteen different plants, ſeparately expoſed in forty ounce meaſures of the water of the river Schuyltrill, produced about ten drachm meaſures of air, the principal part of which was azotic gas; whereas the ſame kind of leaves, expoſed in the ſame quantity of the ſame water, impregnated with carbonic acid, yielded ſeventy-ſeven drachm meaſures of oxigenous air, of a very high degree of purity.

Count

Count Rumford's experiments to obtain oxigen from water by folar light.

Count Rumford made an attempt, in the year 1787, to overthrow the doctrine of the purification of the air by plants. His arguments were, that leaves confined in water were in unnatural circumftances, and that pure air could be obtained from other bodies, as fine fpun glafs, raw filk, common cotton, and that of the poplar tree, expofed in water to the light of the fun *.

Remarks on Prieftley's

The ingenious author of Phytologia alfo fays, it may be fufpected that, in many of the experiments of Prieftley and Ingenhouz, the production of vital air might be fimply owing to the action of the fun's light on the water in which the vegetables were immerfed, like that from the filk in the experiments of Count Rumford; and that the fine points or fharp edges of thefe bodies, contributed only to facilitate the liberation of it when expofed to the fun fhine, which thus difoxigenated the water by their united effect.

and Count Rumford's experiments.

The experiments of Count Rumford are far from being fatisfactory. Thirty grains of raw filk, at the end of three days, yielded him but 3¾ cubic inches of air, and fometimes four days elapfed before a fufficient quantity could be collected for an experiment.

Direct experiments

In order to find how much air could be obtained from the "fine points or fharp edges" of certain bodies acting upon water, the following fubftances were expofed one day to the action of folar light, in forty ounce meafures of pump water.

with fibrous bodies, which gave air lefs in quantity and purity than leaves.

Filaments of afbeftos, baked horfe-hair, common cotton, and that of the afclepias Syriaca, the flower panicles of rhus cotinus, the fine hairy plumes of climatis crifpa, the fpikes of panicum glaucum, and charcoal in powder. From each of thefe fubftances, from two to four drachm meafures of pure air were obtained, which devoured nearly two meafures of nitrous air; confequently it was lefs pure than that procured from leaves expofed in the fame water. There was alfo a much fmaller quantity of it; for from eight to nineteen drachm meafures may be obtained in a few hours, by immerfing the leaves of any plant in the fame water, and expofing them to folar light.

Other fources of air.

Some water, without any mixture, will yield oxigenous gas by the combined action of light and heat; and many fubftances placed in water, appear to act merely by raifing its temperature.

* Tranfactions of the Royal Society for 1787.

The

The green vegetable matter, which forms on all bodies, immerſed a conſiderable time in water, might alſo have been one of the ſources of pure air, in ſome of the experiments of Count Rumford.

Secondly. Many philoſophers ſuppoſe, that vegetables yield oxigenous gas by the decompoſition of water. Its hidrogen is ſaid to enter into plants, while its oxigen is ſet at liberty in the form of pure air.

Plants do not decompoſe water; for they do not operate in pure water.

If this opinion was true, oxigenous gas ſhould be obtained by expoſing leaves in boiled, rain, diſtilled, river, or lime water, but this cannot be done.

Thirdly. Some ſuppoſe that vegetables give oxigenous air to animals, and that the latter yield them azotic gas in return, which they devour for food.

Plants do not (as has been ſuppoſed) emit oxigen and abſorb azote;

If this hypotheſis were juſt, atmoſpheric air would be increaſed in purity by confining leaves in it when it contained no fixed air; and its purity might alſo be increaſed, after being previouſly diminiſhed, by an additional quantity of azotic air, in the ſame manner.

A handful of the leaves of euphorbia picta, nicotiana tobacco, buxus vulgaris, cinna glauca, mimoſa julibreſcens, jaxus procumbens, coryllus avellana, Herculea fœtida, malva criſpa, pinus ſtrobus, colutea arboreſcens, and epilobium, were ſeparately expoſed four hours to the light of the ſun, in forty ounce meaſures of atmoſpheric air, and its purity was found to be neither increaſed nor diminiſhed. After they had remained ſixteen hours in the air, no effect was produced on it. The leaves were freſh gathered, and no decay could be obſerved upon any part of them.

for freſh leaves do not affect atmoſpheric air,

When leaves are plucked promiſcuouſly, and are placed in atmoſpheric air either in the day or night, they diminiſh its purity. Wherever a leaf is perforated, and this is very generally done by inſects, let the perforation be ever ſo ſmall, the part decays, and the coal of this decayed part uniting to the oxigen of the atmoſpheric air, generates carbonic acid, which leſſens its purity.

though wounded or decaying leaves do.

The

Experiments in solar light;

The following table shews the effect of the leaves of plants gathered promiscuously, exposed five hours to the light of the sun, in forty ounce measures of atmospherical air, at a temperature of 75° of Fahrenheit.

A small handful of the	Fixed Air.	Atmospheric Air of the St. and Ard. of 100.
Leaves of Datura strammonium - -	3	96
Rododendron maximum -	5	87
Apium petroselinum - -	4	86
Anthemis nobilis - -	0	100
Sophora australis - -	2	95
Sedum telephium - -	0	100
Amaranthus hybridus - -	10	70

in the dark.

The following table will shew the effects produced in one night, on forty ounce measures of atmospheric air of the purity of 100, by a small handful of leaves gathered promiscuously from a variety of plants.

	Fixed Air.	Atmos. Air.
Leaves of Ilex aquifolium - - -	5	88
Juniperus officinalis - -	4	93
Berberis vulgaris - -	2	86
Franklinia alatamaha - -	3	85
Rododendron maximum -	1	95
Annona triloba - - -	2	88
Buxus vulgaris - - -	2	90
Pinus strobus - - -	2	88
Mitchella repens - -	0	100
Arclepias Syriaca - -	5	86
Hamamelis Virginia - -	0	100
Bignonia radicans - -	3	77
Xanthoriza tinctoria - -	1	94
Magnolia tripetala - -	5	67
Kalmia latifolia - - -	2	85
Pinus picea - - -	3	80
Siriodendron tulipifera -	10	65

According to some philosophers, carbonic acid gas is secreted by certain vegetables in the night; but as the quantity of this air obtained is always in proportion to the decayed parts of plants,

plants, and to the temperature to which they are ſubjected, it appears more rational to aſcribe the generation of it to the coal of the decayed parts uniting with the oxigen of the air in which they are placed.

Leaves expoſed to ſunſhine in a mixture of atmoſpheric air and azote, produced no effect.

To determine whether plants would abſorb or devour azotic gas, eight ounce meaſures of this air were mixed with thirty-two ounce meaſures of atmoſpheric air, ſo that its purity was reduced from 100 to 91. A handful of the leaves of euphorbia picta and coryllus avellana were ſeparately confined in forty ounce meaſures of this air, and expoſed to the influence of a bright ſolar light five hours. No carbonic acid gas was generated, and the purity of the air was exactly the ſame as when firſt tried. No decayed portion could be obſerved upon theſe leaves.

Leaves do not purify the atmoſphere by decompoſing its carbonic gas; becauſe the quantity of this gas is very minute,

As it is acknowledged that the leaves, ſtems, and roots of plants, ſeparate the oxigen from carbonic acid, it may be ſaid, that the oxigenous portion of atmoſpheric air is ſupplied by the decompoſition of this gas, as it is always found in the atmoſphere, and often in water in which vegetables grow.

The quantity of carbonic acid gas in atmoſpheric air, is reckoned to be about one part in an hundred. It muſt, however, vary in different places. We would expect to find the moſt of it in cities, where it is formed by combuſtion, reſpiration, fermentation, and putrefaction. If one meaſure of the air of any large city is thrown up over lime water in an eudiometer, no milky appearance will be produced, ſo that the quantity of carbonic acid in this air muſt be extremely ſmall. As this gas is alſo ſeized upon by alkalis, earths, and metals, and abſorbed by water, the quantity floating in the atmoſphere may be leſs than one part in ten thouſand.

and plants deteriorate the air much more.

When we conſider likewiſe, that the oxigen is never ſeparated from the carbonic acid by leaves, but when they are expoſed, in contact with it, to the light of the ſun; and that every perforation made in a living leaf, however minute, by an inſect, cauſes the part to decay, and abſorb oxigen by day and by night; and that, in the autumn in ſome countries, all leaves fall on the ground, ferment or putrify, and thus diminiſh the purity of common air; and that the petals and fruit of vegetables have the ſame effect, we muſt pronounce, that the oxigenous portion of atmoſpheric air cannot be ſupplied by vegetation.

Air bladders of various plants contain air worſe than that of the atmoſphere.

Dr. Darwin ſuppoſes, that the air in the air bladders of vegetables ſerve to oxigenate the ſeed. The air of the air bladders of cardioſpermum halicacabum, ſtaphylia trifoliata, colutea arboreſcens, and ſophora auſtralis being examined, was found to be a little worſe than the air of the atmoſphere.

III.

On the Theory of Light and Colours. By THOMAS YOUNG, *M. D. F. R. S. Profeſſor of Natural Philoſophy in the Royal Inſtitution.*

(Concluded from page 90.)

PROPOSITION IV.

Prop. IV. Partial reflection of undulations at the confine of mediums differing in denſity.

When an Undulation arrives at a Surface which is the Limit of Mediums of different Denſities, a partial Reflection takes place, proportionate in Force to the Difference of the Denſities.

THIS may be illuſtrated, if not demonſtrated, by the analogy of elaſtic bodies of different ſizes. "If a ſmaller elaſtic body ſtrikes againſt a larger one, it is well known that the ſmaller is reflected more or leſs powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light paſs from a rarer to a denſer ſtratum of ether; and frequently an echo when a ſound ſtrikes againſt a cloud. A greater body ſtriking a ſmaller one, propels it, without loſing all its motion: thus, the particles of a denſer ſtratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting ſubſtance with equal force; and thus a reflection is always ſecondarily produced, when the rays of light paſs from a denſer to a rarer ſtratum." (Phil. Tranſ. for 1800, p. 127.) But it is not abſolutely neceſſary to ſuppoſe an attraction in the latter caſe, ſince the effort to proceed would be propagated backwards without it, and the undulation would be reverſed, a rarefaction returning in place of a condenſation; and this will perhaps be found moſt conſiſtent with the phenomena.

PROPOSI-

PROPOSITION V.

When an Undulation is transmitted through a Surface terminating different Mediums, it proceeds in such a Direction, that the Sines of the Angles of Incidence and Refraction are in the constant Ratio of the Velocity of Propagation in the two Mediums.

Prop. V. Law of refraction of undulations transmitted through the confine of different mediums.

(Barrow, Lect. Opt. II. p. 4. Huygens, *de la Lum.* cap. 3. Euler, *Conj. Phys.* Phil. Transf. for 1800, p. 128. Young's Syllabus. Art. 382.)

Corollary 1. The same demonstrations prove the equality of the angles of reflection and incidence.

Corollary 2. It appears from experiments on the refraction of condensed air, that the ratio of the difference of the sines varies simply as the density. Hence it follows, by Schol. I. Prop. I. that the excess of the density of the ethereal medium is in the duplicate ratio of the density of the air; each particle co-operating with its neighbours in attracting a greater portion of it.

PROPOSITION VI.

When an Undulation falls on the Surface of a rarer Medium, so obliquely that it cannot be regularly refracted, it is totally reflected, at an Angle equal to that of its Incidence. (Phil. Transf. for 1800, p. 128.)

Prop. VI. Total reflection of undulation falling obliquely on the surface of a rarer medium.

Corollary. This phenomenon tends to prove the gradual increase and diminution of density at the surface terminating two mediums, as supposed in hypothesis IV; although Huygens has attempted to explain it somewhat differently.

PROPOSITION VII.

If equidistant Undulations be supposed to pass through a Medium, of which the Parts are susceptible of permanent Vibrations somewhat slower than the Undulations, their Velocity will be somewhat lessened by this vibratory Tendency; and, in the same Medium, the more, as the Undulations are more frequent.

Prop. VI. Undulations are retarded by passing through a medium vibrating with less frequency.

For, as often as the state of the undulation requires a change in the actual motion of the particle which transmits it, that change will be retarded by the propensity of the particle to continue its motion somewhat longer: and this retardation will be more frequent, and more considerable, as the diffe-

rence between the periods of the undulation and of the natural vibration is greater.

Doctrine of heat confifting in vibrations.

Corollary. It was long an eftablifhed opinion, that heat confifts in vibrations of the particles of bodies, and is capable of being tranfmitted by undulations through an apparent vacuum. (Newt. Opt. Qu. 18.) This opinion has been of late very much abandoned. Count Rumford, Profeffor Pictet, and Mr. Davy, are almoft the only authors who have appeared to favour it; but it feems to have been rejected without any good grounds, and will probably very foon recover its popularity.

Suppofe the parts of all bodies to vibrate;

Let us fuppofe that thefe vibrations are lefs frequent than thofe of light; all bodies therefore are liable to permanent vibrations flower than thofe of light; and indeed almoft all are liable to luminous vibrations, either when in a ftate of ignition, or in the circumftances of folar phofphori; but much lefs eafily, and in a much lefs degree, than to the vibrations of heat.

then the more frequent luminous undulations will be moft retarded. Blue light will be moft refracted, and *radiant heat leaft of all,*

It will follow from thefe fuppofitions, that the more frequent luminous undulations will be more retarded than the lefs frequent; and confequently, that blue light will be more refrangible than red, and radiant heat leaft of all; a confequence which coincides exactly with the highly interefting experiments of Dr. Herfchel. (Phil. Tranf. for 1800, p. 284.) It may alfo be eafily conceived, that the actual exiftence of a ftate of flower vibration may tend ftill more to retard the more frequent undulations, and that the refractive power of folid bodies may be fenfibly increafed by an increafe of temperature, as it actually appears to have been in Euler's experiments. (Acad. de Berlin. 1762. p. 328.)

and refractive power will increafe with the temperature.

Scholium. If, notwithftanding, this propofition fhould appear to be infufficiently demonftrated, it muft be allowed to be at leaft equally explanatory of the phenomena with any thing that can be advanced on the other fide, from the doctrine of projectiles; fince a fuppofed accelerating force muft act in fome other proportion than that of the bulk of the particles; and, if we call this an elective attraction, it is only veiling under a chemical term, our incapacity of affigning a mechanical caufe. Mr. Short, when he found by obfervation the equality of the velocity of light of all colours, felt the objection fo forcibly, that he immediately drew an inference from it in favour of the undulatory fyftem. It is affumed in the

This pofition explains the phenomenon at leaft as well as the projectile hypothefis.

propofition,

propofition, that when light is difperfed by refraction, the corpufcles of the refracting fubftance are in a ftate of actual alternate motion, and contribute to its tranfmiffion; but it muft be confeffed, that we cannot at prefent form a very decided and accurate conception of the forces concerned in maintaining thefe corpufcular vibrations.

PROPOSITION VIII.

When two Undulations, from different Origins, coincide either perfectly or very nearly in Direction, their joint Effect is a Combination of the Motions belonging to each.

Prop. VIII. Undulations that coincide in direction will combine.

Since every particle of the medium is affected by each undulation, wherever the directions coincide, the undulations can proceed no otherwife than by uniting their motions, fo that the joint motion may be the fum or difference of the feparate motions, accordingly as fimilar or diffimilar parts of the undulations are coincident.

I have, on a former occafion, infifted at large on the application of this principle to harmonics; (Phil. Tranf. for 1800, p. 130.) and it will appear to be of ftill more extenfive utility in explaining the phenomena of colours. The undulations which are now to be compared are thofe of equal frequency. When the two feries coincide exactly in point of time, it is obvious that the united velocity of the particular motions muft be greateft, and, in effect at leaft, double the feparate velocities; and alfo, that it muft be fmalleft, and if the undulations are of equal ftrength, totally deftroyed, when the time of the greateft direct motion belonging to one undulation coincides with that of the greateft retrograde motion of the other. In intermediate ftates, the joint undulation will be of intermediate ftrength; but by what laws this intermediate ftrength muft vary, cannot be determined without further data. It is well known that a fimilar caufe produces in found, that effect which is called a beat; two feries of undulations of nearly equal magnitude co-operating and deftroying each other alternately, as they coincide more or lefs perfectly in the times of performing their refpective motions.

Effects when they are of equal frequency.

The *beat* in found.

COROLLARY 1. *Of the Colours of ftriated Surfaces.*

Boyle appears to have been the firft that obferved the colours of fcratches on polifhed furfaces. Newton has not noticed

Colours of ftriated furfaces explained.

ticed them. Mazeas and Mr. Brougham have made ſome experiments on the ſubject, yet without deriving any ſatisfactory concluſion. But all the varieties of theſe colours are very eaſily deduced from this propoſition.

by the greater length of ray (or line of undulation) from the depreſſed portion of ſurface,

Let there be in a given plane two reflecting points very near each other, and let the plane be ſo ſituated that the reflected image of a luminous object ſeen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with reſpect to both points, conſidering them as capable of reflecting in all directions. Let one of the points be now depreſſed below the given plane; then the whole path of the light reflected from it, will be lengthened by a line which is to the depreſſion of the point as twice the coſine of incidence to the radius. Fig. 2. Plate VI.

which will occaſion alternations of intenſity and remiſſion in the compound undulation; accordingly as the depreſſion cauſes the undulations of the ſucceeding ray to coincide with or oppoſe thoſe of the preceding ray.

If, therefore, equal undulations of given dimenſions be reflected from two points, ſituated near enough to appear to the eye but as one, wherever this line is equal to half the breadth of a whole undulation, the reflection from the depreſſed point will ſo interfere with the reflection from the fixed point, that the progreſſive motion of the one will coincide with the retrograde motion of the other, and they will both be deſtroyed; but, when this line is equal to the whole breadth of an undulation, the effect will be doubled; and when to a breadth and a half, again deſtroyed; and thus for a conſiderable number of alternations: and, if the reflected undulations be of different kinds, they will be variouſly affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of retardation.

Experiment by parallel ſtrokes;

In order that the effect may be the more perceptible, a number of pairs of points muſt be united into two parallel lines; and, if ſeveral ſuch pairs of lines be placed near each other, they will facilitate the obſervation. If one of the lines be made to revolve round the other as an axis, the depreſſion below the given plane will be as the ſine of the inclination; and, while the eye and luminous object remain fixed, the difference of the length of the paths will vary as this ſine.

ſuch as the fine micrometers of *Coventry* at the

The beſt ſubjects for the experiment are Mr. Coventry's exquiſite micrometers; ſuch of them as conſiſt of parallel lines drawn

drawn on glafs, at the diftance of one five hundredth of an inch, are the moft convenient. Each of thefe lines appears under a microfcope to confift of two or more finer lines, exactly parallel, and at the diftance of fomewhat more than a twentieth of that of the adjacent lines. I placed one of thefe fo as to reflect the fun's light at an angle of 45°, and fixed it in fuch a manner, that while it revolved round one of the lines as an axis, I could meafure its angular motion; and I found, that the brighteft red colour occurred at the inclinations $10\frac{1}{4}°$, $20\frac{1}{4}°$, 32°, and 45°; of which the fines are as the numbers 1, 2, 3, and 4. At all other angles alfo, when the fun's light was reflected from the furface, the colour vanifhed with the inclination, and was equal at equal inclinations on either fide.

diftance of one five hundredth of an inch.

When the angular pofition of thefe is varied they produce colours,

This experiment affords a very ftrong confirmation of the theory. It is impoffible to deduce any explanation of it from any hypothefis hitherto advanced; and I believe it would be difficult to invent any other that would account for it. There is a ftriking analogy between this feparation of colours, and the production of a mufical note by fucceffive echoes from equidiftant iron palifades; which I have found to correfpond pretty accurately with the known velocity of found, and the diftances of the furfaces.

which ftrongly and exclufively confirm the theory.

It is analogous to the mufical echo from iron rails.

It is not improbable that the colours of the integuments of fome infects, and of fome other natural bodies, exhibiting in different lights the moft beautiful verfatility, may be found to be of this defcription, and not to be derived from thin plates. In fome cafes, a fingle fcratch or furrow may produce fimilar effects, by the reflection of its oppofite edges.

Verfatile colours of fome infects, &c. probably of this nature.

COROLLARY 2. *Of the Colours of thin Plates.*

When a beam of light falls on two parallel refracting furfaces, the partial reflections coincide perfectly in direction; and, in this cafe, the interval of retardation, taken between the furfaces, is to their diftance as twice the cofine of the angle of refraction to the radius. For, in Fig. 3, Plate VI. drawing A B and C D perpendicular to the rays, the times of paffing through B C and A D will be equal, and D E will be half the interval of retardation; but D E is to C E as the fine of D C E to the radius. Hence, that D E may be conftant, or that the fame colour may be reflected, the thicknefs C E muft vary as the fecant of the angle of refraction C E D: which agrees

Colours of thin plates explained

by the greater length of ray reflected from the pofterior furface, which muft produce an effect

ſimilar to that of the ſtriated ſurfaces; namely colours,

agrees exactly with Newton's experiments; for the correction is perfectly inconſiderable.

Let the medium between the ſurfaces be rarer than the ſurrounding mediums; then the impulſe reflected at the ſecond ſurface, meeting a ſubſequent undulation at the firſt, will render the particles of the rarer medium capable of wholly ſtopping the motion of the denſer, and deſtroying the reflection, (PROP. IV.) while they themſelves will be more ſtrongly propelled than if they had been at reſt; and the tranſmitted light will be increaſed. So that the colours by reflection will be deſtroyed, and thoſe by tranſmiſſion rendered more vivid, when the double thickneſſes, or intervals of retardation, are any multiples of the whole breadths of the undulations; and, at intermediate thickneſſes the effects will be reverſed; according to the Newtonian obſervations.

which will be tranſmitted and reflected by turns if the thickneſs gradually vary, as in the Newtonian rings.

If the ſame proportions be found to hold good with reſpect to thin plates of a denſer medium, which is indeed not improbable, it will be neceſſary to adopt the corrected demonſtration of PROP. IV. but, at any rate, if a thin plate be interpoſed between a rarer and a denſer medium, the colours by reflection and tranſmiſſion may be expected to change places.

Breadth and duration of the colorific undulations determined,

From Newton's meaſures of the thickneſſes reflecting the different colours, the breadth and duration of their reſpective undulations may be very accurately determined; although it is not improbable, that when the glaſſes approach very near, the atmoſphere of ether may produce ſome little irregularity. The whole viſible ſpectrum appears to be compriſed within the ratio of three to five, or a major ſixth in muſic; and the undulations of red, yellow, and blue, to be related in magnitude as the numbers 8, 7, and 6; ſo that the interval from red to blue is a fourth. The abſolute frequency expreſſed in numbers is too great to be diſtinctly conceived, but it may be better imagined by a compariſon with ſound.

and compared with thoſe of ſound.

If a chord ſounding the tenor $\bar{c}$, could be continually biſected 40 times, and ſhould then vibrate, it would afford a yellow green light: this bring denoted by $\overset{41}{c}$, the extreme red would be $\overset{40}{a}$, and the blue $\overset{41}{d}$. The abſolute length and frequency of each vibration is expreſſed in the table; ſuppoſing light to travel in $8\frac{1}{8}$ minutes 500,000,000,000 feet.

Table of colorific undulations of the ether.

Colours.	Length of an Undulation in parts of an Inch, in Air.	Number of Undulations in an Inch.	Number of Undulations in a Second.
Extreme -	.0000266	37640	463 millions of millions.
Red - -	.0000256	39180	482
Intermediate	.0000246	40720	501
Orange - -	.0000240	41610	512
Intermediate	.0000235	42510	523
Yellow - -	.0000227	44000	542
Intermediate	.0000219	45600	561 (= 2^{48} nearly)
Green - -	.0000211	47460	584
Intermediate	.0000203	49320	607
Blue - -	.0000196	51110	629
Intermediate	.0000189	52910	652
Indigo - -	.0000185	54070	665
Intermediate	.0000181	55240	680
Violet - -	.0000174	57490	707
Extreme -	.0000167	59750	735

Quotation from Robert Hooke exhibiting a similar doctrine.

Scholium. It was not till I had ſatisfied myſelf reſpecting all theſe phenomena, that I found in Hooke's Micrographia, a paſſage which might have led me earlier to a ſimilar concluſion. "It is moſt evident that the reflection from the under or further ſide of the body, is the principal cauſe of the production of theſe colours.—Let the ray fall obliquely on the thin plate, part therefore is reflected back by the firſt ſuperficies, —part refracted to the ſecond ſurface,—whence it is reflected and refracted again.—So that, after two refractions and one reflection, there is propagated a kind of fainter ray—," and, "by reaſon of the time ſpent in paſſing and repaſſing,—this fainter pulſe comes behind the" former reflected "pulſe; ſo that hereby, (the ſurfaces being ſo near together that the eye cannot diſcriminate them from one,) this confuſed or duplicated pulſe, whoſe ſtrongeſt part precedes, and whoſe weakeſt follows, does produce on the retina, the ſenſation of a yellow. If theſe ſurfaces are further removed aſunder, the weaker pulſe may become coincident with the" reflection of the ſecond," or next following pulſe, from the firſt ſurface, "and lagg behind that alſo, and be coincident with the third, fourth, fifth, ſixth, ſeventh, or eighth—; ſo that, if there be a thin tranſparent body, that from the greateſt thinneſs requi-

ſite,

ſite to produce colours, does by degrees grow to the greateſt thickneſs, the colours ſhall be ſo often repeated, as the weaker pulſe does loſe paces with its primary or firſt pulſe, and is coincident with a" ſubſequent "pulſe. And this, as it is coincident, or follows from the firſt hypotheſis I took of colours, ſo upon experiment have I found it in multitudes of inſtances that ſeem to prove it." (P. 65—67.) This was printed about ſeven years before any of Newton's experiments were made. We are informed by Newton, that Hooke was afterwards diſpoſed to adopt his "ſuggeſtion" of the nature of colours; and yet it does not appear that Hooke ever applied that improvement to his explanation of theſe phenomena, or inquired into the neceſſary conſequence of a change of obliquity, upon his original ſuppoſition, otherwiſe he could not but have diſcovered a ſtriking coincidence with the meaſures laid down by Newton from experiment. All former attempts to explain the colours of thin plates, have either proceeded on ſuppoſitions which, like Newton's, would lead us to expect the greateſt irregularities in the direction of the refracted rays; or, like Mr. Michell's, would require ſuch effects from the change of the angle of incidence, as are contrary to the effects obſerved; or they are equally deficient with reſpect to both theſe circumſtances, and are inconſiſtent with the moſt moderate attention to the principal phenomena.

which was printed ſeven years before Newton made his experiments.

COROLLARY 3. *Of the Colours of thick Plates.*

When a beam of light paſſes through a refracting ſurface, eſpecially if imperfectly poliſhed, a portion of it is irregularly ſcattered, and makes the ſurface viſible in all directions, but moſt conſpicuouſly in directions not far diſtant from that of the light itſelf: and, if a reflecting ſurface be placed parallel to the refracting ſurface, this ſcattered light, as well as the principal beam, will be reflected, and there will alſo be a new diſſipation of light, at the return of the beam through the refracting ſurface. Theſe two portions of ſcattered light will coincide in direction; and, if the ſurfaces be of ſuch a form as to collect the ſimilar effects, will exhibit rings of colours. The interval of retardation is here, the difference between the paths of the principal beam and of the ſcattered light between the two ſurfaces; of courſe, wherever the inclination of the ſcattered light is equal to that of the beam, although in diffe-

Colours of thick plates explained from the greater length of ſuch rays of ſcattered light (compared with the principal ray) as paſs through the firſt ſurface, and are reflected from the ſecond with a different inclination.

rent planes, the interval will vaniſh, and all the undulations will conſpire. At other inclinations, the interval will be the difference of the ſecants from the ſecant of the inclination or angle of refraction of the principal beam. From theſe cauſes, all the colours of concave mirrors obſerved by Newton and others are neceſſary conſequences: and it appears that their production, though ſomewhat ſimilar, is by no means, as Newton imagined, identical with the production of thoſe of thin plates.

Corollary 4. *Of Blackneſs.*

Blackneſs produced by innumerable reflections at the confine of two ſurfaces where the ether gradually varies in denſity: the effect of which is, that the undulations confound and deſtroy each other, and conſequently the ſenſation of light.

In the three preceding corollaries, we have conſidered the refracting and reflecting ſubſtances as limited by a mathematical ſurface; but this is perhaps never phyſically true. The ethereal atmoſpheres may extend on each ſide the ſurface as far as the breadth of one or more undulations; and, if they be ſuppoſed to vary equally in denſity at every part, the partial reflections from each of the infinite number of ſurfaces, where the denſity changes, will very much interfere with each other, and deſtroy a conſiderable portion of the reflected light, ſo that the ſubſtance may become poſitively black; and this effect may take place in a greater or leſs degree, as the denſity of the ethereal atmoſphere varies more or leſs equably; and, in ſome caſes, particular undulations being more affected than others, a tinge of colour may be produced. Accordingly, M. Bouguer has obſerved a conſiderable loſs of light, and in ſome inſtances a tinge of colour, in total reflections at the ſurface of a rarer medium.

Corollary 5. *Of Colours by Inflection.*

Colours by inflection explained by the different lengths of the bended rays occaſioning alternations in the compound undulations as in Corollary 1.

Whatever may be the cauſe of the inflection of light paſſing through a ſmall aperture, the light neareſt its centre muſt be the leaſt diverted, and the neareſt to its ſides the moſt; another portion of light falling very obliquely on the margin of the aperture, will be copiouſly reflected in various directions; ſome of which will either perfectly or very nearly coincide in direction with the unreflected light, and, having taken a circuitous route, will ſo interfere with it, as to cauſe an appearance of colours. The length of the two tracks will differ the leſs, as the direction of the reflected light has been leſs changed by its reflection, that is, in the light paſſing neareſt to

to the margin; ſo that the blues will appear in the light neareſt the ſhadow. The effect will be increaſed and modified, when the reflected light falls within the influence of the oppoſite edge, ſo as to interfere with the light ſimply inflected by that alſo.

More particular examination of the conſequences.

But, in order to examine the conſequences more minutely, it will be convenient to ſuppoſe the inflection cauſed by an ethereal atmoſphere, of a denſity varying as a given power of the diſtance from a centre, as in the eighth propoſition of the laſt Bakerian Lecture. (Phil. Tranſ. for 1801, p. 83.) Putting $r = 3$, and $x = \frac{1}{2}$, I have conſtructed a diagram, (Fig. 4,) which ſhows, by the two pair of curves, the relative poſition of the reflected and unreflected portions of any one undulation at two ſucceſſive times, and alſo, by ſhaded lines drawn acroſs, the parts where the intervals of retardation are in arithmetical progreſſion, and where ſimilar colours will be exhibited at different diſtances from the inflecting ſubſtance. The reſult fully agrees with the obſervations of Newton's third book, and with thoſe of later writers. But I do not conſider it as quite certain, until further experiments have been made on the inflecting power of different ſubſtances, that Dr. Hooke's explanation of inflection, by the tendency of light to diverge, may not have ſome pretenſions to truth. I am ſorry to be obliged to recall here the aſſent which, at firſt ſight, I was induced to give to a ſuppoſed improvement of a late author. (Phil. Tranſ. for 1800, p. 128.)

Scholium. In the conſtruction of the diagram, it becomes neceſſary to find the time ſpent by each ray in its paſſage. Since the velocity was denoted by $x^{-\frac{1}{r}}$, on the ſuppoſition of a projectile, it will be as $x^{\frac{1}{r}}$ on the contrary ſuppoſition, (Phil. Tranſ. for 1801, p. 27. *Schol.* 2. Prop. I.) and the fluxion of the diſtance deſcribed being $\frac{\dot{x}}{\sqrt{1-yy}}$, that of the time will be $\frac{x^{-\frac{1}{r}}\dot{x}}{\sqrt{1-yy}}$. or $\frac{rs}{1-r}$. $\frac{\dot{y}}{yy\cdot\sqrt{1-yy}}$, of which the fluent is $\frac{r}{1-r}\cdot\frac{s}{y}\cdot\sqrt{1-yy}$. Therefore, with the radius $x^{1-\frac{1}{r}}$, deſcribe

defcribe a circle concentric with the furfaces of the inflecting atmofphere, then the angle defcribed by the ray during its paffage through the atmofphere, will always be to the angle fubtended by the line cut off by this circle from the incident ray produced, in the ratio of r to $r-i$; and the time fpent in this paffage, will be in the fame ratio to the time that would have been fpent in defcribing this intercepted portion with the initial velocity. For y, being equal to $s x^{\frac{1}{r}-1}$, is the fine of the inclination of the incident ray to the radius, where it meets this circle; therefore by the propofition quoted, the angle defcribed is in a given ratio to the angle at the centre, which is the difference of the inclinations. Making $x^{1-\frac{1}{r}}$ or $\frac{s}{y}$ radius, the fine, inftead of y, becomes s, and the cofine $\sqrt{\frac{ss}{yy}-ss}$, or $\frac{s}{y}\sqrt{1-yy}$, and, when $y=ss$, $\sqrt{1-ss}$; therefore the line intercepted is to the difference of the fluents as r to $r-1$. (See alfo Young's Syllabus, Art. 372.)

PROPOSITION IX.

Radiant Light confifts in Undulations of the luminiferous Ether.

Conclufion. Radiant light confifts in undulations of the ether.

This propofition is the general conclufion from all the preceding; and it is conceived that they confpire to prove it in as fatisfactory a manner as can poffibly be expected from the nature of the fubject. It is clearly granted by Newton, that there are undulations, yet he denies that they conftitute light; but it is fhown in the three firft Corollaries of the laft Propofition, that all cafes of the increafe or diminution of light are referable to an increafe or diminution of fuch undulations, and that all the affections to which the undulations would be liable, are diftinctly vifible in the phenomena of light; it may therefore be very logically inferred, that the undulations are light.

A few detached remarks will ferve to obviate fome objections which may be raifed againft this theory.

Remarks. Newton without giving a reafon rejected the law of Huygens in explaining the phenomena of Iceland cryftal.

1. Newton has advanced the fingular refraction of the Iceland cryftal, as an argument that the particles of light muft be projected corpufcles; fince he thinks it probable that the different fides of thefe particles muft be differently attracted by the cryftal, and fince Huygens has confeffed his inability to account

account in a satisfactory manner for all the phenomena. But, contrarily to what might have been expected from Newton's usual accuracy and candour, he has laid down a new law for the refraction, without giving a reason for rejecting that of Huygens, which Mr. Hauy has found to be more accurate than Newton's; and, without attempting to deduce from his own system any explanation of the more universal and striking effects of doubling spars, he has omitted to observe that Huygens's most elegant and ingenious theory perfectly accords with these general effects, in all particulars, and of course derives from them additional pretensions to truth: this he omits, in order to point out a difficulty, for which only a verbal solution can be found in his own theory, and which will probably long remain unexplained by any other.

Michell's experiments on the momentum of light explained, without supposing any projection.

2. Mr. Michell has made some experiments, whioh appear to show that the rays of light have an actual momentum, by means of which a motion is produced when they fall on a thin plate of copper delicately suspended. (Priestley's Optics.) But, taking for granted the exact perpendicularity of the plate, and the absence of any ascending current of air, yet since, in every such experiment, a greater quantity of heat must be communicated to the air at the surface on which the light falls than at the opposite surface, the excess of expansion must necessarily produce an excess of pressure on the first surface, and a very perceptible recession of the plate in the direction of the light. Mr. Bennet has repeated the experiment, with a much more sensible apparatus, and also in the absence of air; and very justly infers from its total failure, an argument in favour of the undulatory system of light. (Phil. Trans. for 1792, p. 87.) For, granting the utmost imaginable subtility of the corpuscles of light, their effects might naturally be expected to bear some proportion to the effects of the much less rapid motions of the electrical fluid, which are so very easily perceptible, even in their weakest states.

They did not succeed with Bennet.

Latent light and heat not inconsistent with the doctrine of vibrations.

3. There are some phenomena of the light of solar phosphori, which at first sight might seem to favour the corpuscular system; for instance, its remaining many months as if in a latent state, and its subsequent re-emission by the action of heat. But, on further consideration, there is no difficulty in supposing the particles of the phosphori which have been made to vibrate by the action of light, to have this action abruptly suspended

ſuſpended by the intervention of cold, whether as contracting the bulk of the ſubſtance or otherwiſe; and again, after the reſtraint is removed, to proceed in their motion, as a ſpring would do which had been held faſt for a time in an intermediate ſtage of its vibration; nor is it impoſſible that heat itſelf may, in ſome circumſtances, become in a ſimilar manner latent. (Nicholſon's Journal, Vol. II. p. 399.) But the affections of heat may perhaps hereafter be rendered more intelligible to us; at preſent, it ſeems highly probable that light differs from heat only in the frequency of its undulations or vibrations; thoſe undulations which are within certain limits, with reſpect to frequency, being capable of affecting the optic nerve, and conſtituting light; and thoſe which are ſlower, and probably ſtronger, conſtituting heat only; that light and heat occur to us, each in two predicaments, the vibratory or permanent, and the undulatory or tranſient ſtate; vibratory light being the minute motion of ignited bodies, or of ſolar phoſphori, and undulatory or radiant light the motion of the ethereal medium excited by theſe vibrations; vibratory heat being a motion to which all material ſubſtances are liable, and which is more or leſs permanent; and undulatory heat that motion of the ſame ethereal medium, which has been ſhown by Mr. King, (Morſels of Criticiſm, 1786, p. 99,) and M. Pictet, (*Eſſais de Phyſique*, 1790, alſo in Sauſſure's Voyage dans les Alpes, 1786.) to be as capable of reflection as light, and by Dr. Herſchel to be capable of ſeparate refraction. (Phil. Tranſ. for 1800, p. 284,) How much more readily heat is communicated by the free acceſs of colder ſubſtances, than either by radiation or by tranſmiſſion through a quieſcent medium, has been ſhown by the valuable experiments of Count Rumford. It is eaſy to conceive that ſome ſubſtances, permeable to light, may be unfit for the tranſmiſſion of heat, in the ſame manner as particular ſubſtances may tranſmit ſome kinds of light, while they are opaque with reſpect to others.

Light and heat differ only in the frequency of undulation or vibration,

capable in both caſes of the ſame modifications.

On the whole it appears, that the few optical phenomena which admit of explanation by the corpuſcular ſyſtem, are equally conſiſtent with this theory; that many others, which have long been known, but never underſtood, become by theſe means perfectly intelligible; and that ſeveral new facts are found to be thus only reducible to a perfect analogy with other facts, and to the ſimple principles of the undulatory ſyſtem.

This theory explains all that is ſolved by the corpuſcular ſyſtem, and much more.

ſyſtem. It is preſumed, that henceforth the ſecond and third books of Newton's Optics will be conſidered as more fully underſtood than the firſt has hitherto been; but, if it ſhould appear to impartial judges, that additional evidence is wanting for the eſtabliſhment of the theory, it will be eaſy to enter more minutely into the details of various experiments, and to ſhow the inſuperable difficulties attending the Newtonian doctrines, which, without neceſſity, it would be tedious and invidious to enumerate. The merits of their author in natural philoſophy, are great beyond all conteſt or compariſon; his optical diſcovery of the compoſition of white light, would alone have immortaliſed his name; and the very arguments which tend to overthrow his ſyſtem, give the ſtrongeſt proofs of the admirable accuracy of his experiments.

Experiment in which the corpuſcular theory ought to exhibit diſtortion.

Sufficient and deciſive as theſe arguments appear, it cannot be ſuperfluous to ſeek for further confirmation; which may with conſiderable confidence be expected, from an experiment very ingeniouſly ſuggeſted by Profeſſor Robiſon, on the refraction of the light returning to us from the oppoſite margins of Saturn's ring; for, on the corpuſcular theory, the ring muſt be conſiderably diſtorted when viewed through an achromatic priſm: a ſimilar diſtortion ought alſo to be obſerved in the diſc of Jupiter; but, if it be found that an equal deviation is produced in the whole light reflected from theſe planets, there can ſcarcely be any remaining hope to explain the affections of light, by a compariſon with the motions of projectiles.

IV.

An Analyſis of a Mineral Subſtance from North America, containing a Metal hitherto unknown. By CHARLES HATCHETT, *Eſq.*

(Concluded from Page 138.)

H.

The white precipitate was little affected by diſtillation of the ſulphuric and nitric acids.

THE white precipitate, when diſtilled with four parts of ſulphur, remained pulverulent, and, from white, was only changed to a pale aſh-colour.

Nitric acid was digeſted on the powder, and, being heated, afforded ſome nitrous gas; after this, the powder became white, and in every reſpect recovered its original properties.

Before

I.

Before I conclude this section, I must observe, that when the olive-green precipitates, obtained by prussiate of potash, were digested in an alkaline lixivium, they were decomposed; for the alkali combined with the prussic acid, and with a small part of the white matter; but the greater part of the latter remained undissolved, in the same white flocculent state which was noticed when the alkaline combinations were mentioned.

The precipitates by pruss. pot. were decomposed by humid alkali, which took up the pr. acid and a little of the white matter.

The orange-coloured precipitates, formed by tincture of galls, were also decomposed when digested in boiling nitric acid; and the white matter was recovered in its original state.

§ III. REMARKS.

The preceding experiments shew, that the ore which has been analysed, consists of iron combined with an unknown substance, and that the latter constitutes more than three-fourths of the whole. This substance is proved to be of a metallic nature, by the coloured precipitates which it forms with prussiate of potash, and with tincture of galls; by the effects which zinc produces, when immersed in the acid solutions; and by the colour which it communicates to phosphate of ammonia, or rather to concrete phosphoric acid, when melted with it.

Obs. The ore is iron, with three parts of the white matter.

The latter is metallic; for it is precip. by prussiate, and by galls, and by zinc, and it colours phos. acid by fusion.

Moreover, from the experiments made with the blow-pipe, it seems to be one of those metallic substances which retain oxigen with great obstinacy, and are therefore of difficult reduction.

It is of difficult reduction,

It is an acidifiable metal; for the oxide reddens litmus paper, expels carbonic acid, and forms combinations with the fixed alkalis. But it is very different from the acidifiable metals which have of late been discovered; for,

and acidifiable.

1. It remains white when digested with nitric acid.

2. It is soluble in the sulphuric and muriatic acids, and forms colourless solutions, from which it may be precipitated, in the state of a white flocculent oxide, by zinc, by the fixed alkalies, and by ammonia. Water also precipitates it from the sulphuric solution, in the state of a sulphate.

Differs from all other the acidifiable metals, in the properties here enumerated.

3. Prussiate of potash produces a copious and beautiful olive-green precipitate.

4. Tincture of galls forms orange or deep yellow precipitates.

5. Unlike the other metallic acids, it refuses to unite with ammonia.

6. When mixed and distilled with sulphur, it does not combine with it so as to form a metallic sulphuret.

7. It does not tinge any of the fluxes, except phosphoric acid; with which, even in the humid way, it appears to have a very great affinity.

8. When combined with potash and dissolved in water, it forms precipitates, upon being added to solutions of tungstate of potash, molybdate of potash, cobaltate of ammonia, and the alkaline solution of iron.

These properties completely distinguish it from the other acidifiable metals, *viz.* arsenic, tungsten, molybdena, and chromium; as to the other metals lately discovered, such as uranium, titanium, and tellurium, they are still farther removed from it.

The colours of the precipitates produced by prussiate of potash and tincture of galls, approach the nearest to those afforded by titanium. But the prussiate of the latter is much browner; and the gallate is not of an orange colour, but of a brownish red, inclining to the colour of blood. Besides, even if these precipitates were more like each other, still the obstinacy with which titanium refuses to unite with the fixed alkalis, and the insolubility of it in acids when heated, sufficiently denote the different nature of these two substances.

The iron is in state of brown oxide.

The iron in the ore which has been examined, is apparently in the same state as it is in wolfram, *viz.* brown oxide; and this oxide is mineralized by the metallic acid which has been described, in the same manner as the oxides of iron and manganese are mineralized by the tungstic acid or rather oxide. For, from several experiments made upon a large scale, I have reason to believe that in wolfram, the tungsten has not attained the maximum of oxidation. Several facts in the course of the experiments lately described, seem to prove, that this new metal differs from tungsten and the other acidifiable metals, by a more limited extent of oxidation; for, unlike these, it seems to be incapable of retaining oxigen sufficient to enable the total quantity to combine with the fixed alkalies. In § II. G. 2, this

this is very evident; for, from the experiment there described, it appears, that when the metallic acid or oxide was digested with lixivium of potash, only a part was dissolved; and that the remainder was insoluble in the same lixivium, till it had received an additional portion of oxigen, by being treated with nitric acid; also that several of these alternate operations were required, before any given quantity of the metallic oxide could be completely combined with the alkali. Now there is much reason to believe, that in this case, when the metallic oxide or acid was digested with potash, the portion which was dissolved received an accession of oxigen at the expence of the other part, which of course was thus reduced to the state of an insoluble oxide, and therefore required to be again oxidated by nitric acid, before it could combine with the alkaline solution; but still it appeared, that an adequate proportion of oxigen could never be superinduced, so as to render the oxide totally and immediately soluble in the alkalies by one operation, or even by two.

We may, therefore, regard this as an instance of the effects resulting from disposing affinity, and as very similar to those observed in respect to copper, which have been noticed by my ingenious friend Mr. Chenevix, in his valuable analysis of the arseniates of copper and of iron *.

From the limited researches on the small quantity of ore, the only deduction as to immediate utility is, that the precipitates are fine colours.

My researches into the properties of this metal, have of course been much limited by the smallness of the quantity which I had to operate upon; but I flatter myself that more of the ore may soon be procured from the Massachuset mines, particularly as a gentleman now in England (Mr. Smith, Secretary to the American Philosophical Society), has obligingly offered his assistance on this occasion. We shall then be able more fully to investigate the nature of this substance; and shall be more capable of judging how far it may be applicable to useful purposes. At present, all that can be said is, that the olive-green prussiate and the orange-coloured gallate are fine colours; and, as they do not appear to fade when exposed to light and air, they might probably be employed with advantage as pigments.

The new metals are probably compounds.

I am much inclined to believe, that the time is perhaps not very distant, when some of the newly discovered metals, and

* Phil. Trans. for 1801, p. 233.

other ſubſtances, which are now conſidered as ſimple, primitive, and diſtinct bodies, will be found to be compounds. Yet I only entertain and ſtate this opinion as a probability; for, until an advanced ſtate of chemical knowledge ſhall enable us to compoſe, or at leaſt to decompoſe, theſe bodies, each muſt be claſſed and denominated as a ſubſtance *ſui generis*. Conſidering, therefore, that the metal which has been examined is ſo very different from thoſe hitherto diſcovered, it appeared proper that it ſhould be diſtinguiſhed by a peculiar name; and, having conſulted with ſeveral of the eminent and ingenious chemiſts of this country, I have been induced to give it the name of Columbium.

POSTSCRIPT.

It appears proper to mention ſome unſucceſsful attempts which I have lately made to reduce the white oxide.

Reduction. The white powder was only blackened by the ſtrong heat with charcoal.

Fifty grains were put into a crucible coated with charcoal; and being covered with the ſame, the crucible was cloſely luted, and was expoſed to a ſtrong heat, in a ſmall wind furnace, during about one hour and an half. When the crucible was broken, the oxide was found in a pulverulent ſtate; and, from white, was become perfectly black.

The phoſphuret.

In order to form a phoſphuret, ſome phoſphoric acid was poured upon a portion of the white oxide; and, being evaporated to dryneſs, the whole was put into a crucible coated with charcoal, as above deſcribed. The crucible was then placed in a forge belonging to Mr. Chenevix, and a ſtrong heat was kept up for half an hour.

The incloſed matter was ſpongy, and of a dark brown; it in ſome meaſure reſembled phoſphuret of titanium.

After this we wiſhed to try the effect of a ſtill greater heat; but in this experiment the crucible was melted.

The above experiments ſhew, that the white oxide, like ſeveral other metallic ſubſtances, may be deoxidated to a certain degree, without much difficulty, but that the complete reduction of it is ſtill far from being eaſily effected.

V. On

V.

On the Effect of Sound upon the Barometer. By Sir Henry C. Englefield, *Bart. F. R. S. (From the Journals of the Royal Institution, No. 9.)*

During the time I ſpent at Bruſſels in the year 1773 and 1774, it occurred to me, that the effect of ſound on the barometer had not, to my knowledge, been attended to; and that it was by no means certain, whether that inſtrument was capable of being ſenſibly affected by thoſe elaſtic vibrations cauſed in the atmoſphere, by the percuſſion of a ſonorous body. I thought the idea worthy of being purſued, and the means of making ſatisfactory experiments were moſt opportunely in my power. Whether the barometer be affected by ſonorous undulation.

The ſound of a very large bell appeared to me the moſt powerful, and at the ſame time to be approached with the greateſt ſecurity and eaſe to the obſerver. The exploſion of artillery, beſides the very diſagreeable ſmoke and danger of the recoil, might be objected to, on account of the ſudden production of elaſtic and heated vapour, which might, independent of the ſound, inſtantaneouſly alter the ſtate of the atmoſphere, and thereby lead the obſerver into very great and unavoidable errors. The bell preferred for experiments.

Every one who has been in the Low Countries muſt know, that very large bells, and immenſe numbers of them, are the pride of their churches; and that they are rung quite out, not tolled, on every great feſtival. The great bell of the collegiate church of St. Gudula, at Bruſſels, weighs, as I was told, ſixteen thouſand pounds, and on this I determined to found my experiment. Large bells in the Netherlands.

Two objections only could be made to the reſult of this experiment, the one, that the motion of the bell might cauſe a vibration in the walls of the building, which would hinder the placing the barometer in a ſtate of repoſe; the other, that the ſwinging ſo large a maſs with a conſiderable degree of velocity, might of itſelf agitate the air ſo as to cauſe vibrations in the mercury totally independent of ſound. Whether theſe would produce irregularity by agitating the building or the air.

The ſtrength of the walls of the ſteeple, and the manner of hanging the bell, which was contained in a frame of timber, founded Obſervations.

founded on a ſtrong vault, and totally independent of the wall of the ſteeple, might alone have anſwered the firſt of theſe objections; but happily a moſt complete and ſatisfactory anſwer to both of them, was furniſhed by the manner in which the bell was rung.

Preparation for experiment.

As the bell was to ring out full in an inſtant, at a ſignal given from below, it is neceſſary to have it in motion ſome time beforehand; and during that time, the clapper is fixed to one ſide by a ſtrong ſtick croſſing the mouth of the bell, which, at the ſignal, is pulled out by the hand of a perſon placed for that purpoſe. If then, our barometer ſhewed no variation during all this time, we were abſolutely certain that whatever motion was perceived afterwards, was wholly owing to the ſound.

Mr. Pigott, who was then at Bruſſels, was kind enough to lend me one of his barometers, made by Ramſden, and his ſon made the following obſervations jointly with myſelf:

Narrative.

At two o'clock in the afternoon of the 1ſt of November 1773, we went into the northweſt tower of St. Gudula's church, and having fixed the barometer firmly in the opening of a window, not above ſeven feet from the bottom of the bell, we waited quietly for its ringing.

The height of the mercury before the bell began to ſwing, as obſerved by Mr. Pigott, was 29.478 inches. The bell being in full ſwing, no alteration whatever was perceptible.

The mercury was elevated by the ſound of a large bell.

The inſtant that the clapper was looſed the mercury leaped up, and continued that ſort of ſpringing motion, at every ſtroke of the clapper, during the whole time of the ringing of the bell. Theſe were our obſervations:

During the ringing of the bell, Mr. P. -	29.469
During the ringing, by myſelf.	
Higheſt - - - - - -	29.480
Loweſt - - - - - -	29.474
Higheſt - - - - - -	29.482
Loweſt - - - - - -	29.472

Theſe obſervations were made with the greateſt attention; and, conſidering their delicacy and the difficulty of obſerving, agree very nearly. They appear to give from 6 to 10 thouſandths of an inch for the effect of this ſound on the barometer. It is to be obſerved, that Mr. Pigott, in general, eſtimated the

the height of the mercury about 5 thousandths lower than myself, which brings our observations to a very near agreement. The following observations prove this:

On the top of the tower, Mr. P. - -	29.424
Ditto, by me - - - - -	29.430
At the foot of the tower, Mr. P. - -	29.639
Ditto, by me - - - - - -	29.642
In the court of the English Nuns, by Mr. P.	29.676
Ditto, by me - - - - -	29.682

And I should think that the difference of eyes may frequently cause such a variation among different observers; at least in delicate observations, it will be always prudent to make the experiment.

NOTE BY DR. YOUNG.

THESE observations appear to agree too well with each other, to allow us to doubt of their accuracy. It therefore becomes necessary to inquire into the causes of the different heights of the barometer. It is indeed barely possible, that a sudden stroke of the clapper on the bell might produce a greater agitation of the building than the preceding alternate motion of the bell itself: but this explanation cannot be called satisfactory. It is certain, that there was neither more nor less air in the tower while the bell was sounding, than while it was silent; the mean density of the air could therefore not have been changed; and if the alternate motions of the particles of air which constitute sound, had taken place by equal degrees, and with equal velocities, in each opposite direction, there is no reason to suppose that the increase of pressure on the surface of the mercury, at one instant, could have tended to raise it, more than the decrease of pressure, in the opposite state of the undulation, would have depressed it. But the same consequence does not follow, if we conceive the motion of the air, in advancing, to be more rapid, but of shorter continuance, than its retrogade motion. For if the wind blew for one hour with a velocity of 4, and the same air returned, in the course of two hours, with a velocity of 2, an obstacle upon which it had acted in both directions would not be found in its original place; for the action of the wind upon an obstacle is as the square

Observations by Dr. Young.

The mean density of the air was not changed.

If the motion of advance were more rapid than of return in the undulations, it would have the effect of a pressure.

ſquare of the velocity, and the time would not compenſate for the difference of force. It is therefore eaſy to ſuppoſe, that the law of the bell's vibration was in this experiment ſuch, that the air advanced towards the barometer with a greater velocity than it receded, although for a ſhorter time; and that hence the whole effect was the ſame as if the mean preſſure of the air had been increaſed. Such a law might eaſily reſult from a combination of a more regular principal vibration with one or more ſubordinate ones, in different relations; and ſimilar caſes may ſometimes be obſerved in the vibrations of chords. Here we find a ſlight degree of repulſion, in conſequence of the undulations of an elaſtic medium. Dr. Hooke attempted to explain the phenomena of attraction by means of ſimilar undulations of an ether, which he ſuppoſed to be the medium ſerving for the communication of heat; but it muſt be confeſſed, that the conjecture has little appearance of probability.

This law might eaſily reſult from ſubordinate vibrations, combining with the principal.

Slight repulſions from this cauſe in elaſtic mediums.

Whether gravity be ſo cauſed.

VI.

On the Expanſion of Carbonated Hidrogen by Electricity. From a Correſpondent.

To Mr. NICHOLSON.

SIR,

I VENTURE to trouble you with a few remarks on a phenomenon in chemiſtry, the expanſion of carbonated hidrogen gas on the electric ſpark being paſſed through it. None of the explanations of this circumſtance which I have met with, appear to me to be ſatisfactory; and I have ſtated a few objections which occurred to me on conſidering them. If you think them of ſufficient weight to fill a corner of your ably-conducted Journal, ſome correſpondent of greater abilities may perhaps give a new explanation of the fact. But if you think them beneath your notice, and unworthy of a place among the excellent communications with which your numbers are filled, I have only to beg that you will pardon the preſumption of a very young chemiſt, who, by this firſt attempt, has perhaps only expoſed his own ignorance.

Expanſion of carb. hidrogen by electricity not yet explained.

I am, Sir,

Your's with much reſpect,

Edin. June 8, 1802. G. H.

On

On the Expanſion of carbonated Hidrogen Gas.

This fact was firſt obſerved by Dr. Auſtin. On paſſing the electric ſpark through a quantity of carbonated hidrogen gas, he found that the gas was permanently dilated to more than twice its original bulk. He concluded that this remarkable expanſion could only be owing to the evolution of hidrogen gas. Upon burning the air thus expanded in oxigen gas, he found that it required more oxigen for its combuſtion than an equal quantity of carbonated hidrogen gas, which had not been expanded by the electric ſpark. *An addition therefore had been made to the combuſtible matter;* for the quantity of oxigen neceſſary to complete the combuſtion of any body, is always proportional to the quantity of that body. He concluded from theſe experiments, that he had decompoſed the carbon which had been diſſolved in the hidrogen, and that carbon was compoſed of hidrogen and azote, ſome of which was always found in the veſſel after the combuſtion.

Dr. Auſtin firſt obſerved that carbon. hidrogen is expanded to double by electricity.

His theory that carbon is hidrogen and azote.

If Dr. Auſtin had more attentively conſidered the circumſtances of theſe experiments, he would probably been prevented from drawing this concluſion. *The quantity of combuſtible matter had been increaſed.* Now, if the expanſion of the carbonated hidrogen gas was owing merely to the decompoſition of carbon, no ſuch increaſe ought to have taken place, but rather the contrary; for the carbon, which was itſelf a combuſtible ſubſtance, was reſolved into two ingredients, hidrogen and azote, only the firſt of which burnt on the addition of oxigen, and the application of heat. And beſides, if the carbon had been reſolved into hidrogen and azote, the product of the combuſtion could only have been a greater quantity of water, with a reſiduum of azote; for the hidrogen evolved by the dilatation of the carbonated hidrogen gas, combining with the additional oxigen, muſt have formed an additional quantity of water. But it is a fact, which the Doctor does not ſeem to have attended to, that beſides water, *carbonic acid gas* is produced from the combuſtion of the expanded carbonated hidrogen. Whence, then, comes the carbonic acid gas?

Objections. Hidrogen and azote are not more combuſtible than carbon: and the product is not mere water and azote, but alſo carbonic acid gas.

Mr. Henry *, who repeated Dr. Auſtin's experiments with great accuracy, found that he was correct with regard to the

Mr. Henry found that the expanſion is

* Philoſ. Tranſact. 1797.

produced by hidrogen from decomposed water.

increaſe of combuſtible matter. He alſo found, that the expanſion could not be carried beyond a certain point, about twice the original bulk of the gas. Upon burning ſeparately by means of oxigen, two equal portions of carbonated hidrogen gas, one of which had been expended by electricity to twice its original bulk, the other not, he found that each of them produced preciſely the ſame quantity of carbonic acid gas; a proof, that the carbon in both remained the ſame, and that the hidrogen could not have been produced from it. He concluded therefore, that the evolution of the hidrogen is produced from the decompoſition of the water with which hidrogen is always more or leſs impregnated; to prove which, he took hidrogen gas, from which he expelled as much water as poſſible, and found that the electric ſpark produced a very ſmall dilatation; but on admitting a drop or two of water, the expanſion went on as uſual. It is eaſily ſeen how this decompoſition is effected. Carbon at a high temperature has a greater affinity for oxigen than hidrogen gas; when the temperature is raiſed, therefore, by the electric ſpark, the carbon unites with the oxigen of the water, forming carbonic acid gas, and the hidrogen is evolved. As to the azote, it muſt have been produced from the admiſſion of atmoſpheric air into the proceſs.

But this diſcovery does not account for the greater quantity of oxigen required to burn the expanded gas.

Although this theory of Mr. Henry ſeems to prove the production of hidrogen from the decompoſition of the water contained in the gas; ſtill, however, it is no more able to account for the increaſe of combuſtible matter than that of Dr. Auſtin. When the gas is dilated, hidrogen and oxigen are evolved from the water. The hidrogen goes to increaſe the bulk of the gas, and the oxigen unites with the carbon. Expanded carbonated hidrogen gas, then, contains hidrogen, carbon, and carbonic acid gas. In combuſtion, the evolved hidrogen, in order to form water, requires a quantity of additional oxigen, preciſely the ſame as that with which it was combined before decompoſition of the water: and on the other hand, the carbon requires preciſely the ſame quantity leſs, as part of it is already rendered incombuſtible, by being combined with the oxigen of the water. Therefore, according to Mr. Henry's theory, a quantity of expanded carbonated hidrogen gas requires the ſame quantity of oxigen for its combuſtion, as an equal quantity that has not been expanded: and

and consequently they both contain the same quantity of combustible matter; for in proportion as the combustible matter is increased by the formation of hidrogen, in the same proportion it is diminished by the formation of carbonic acid gas. But this is observed to be contrary to fact; for carbonated hidrogen gas when expanded does actually contain more combustible matter than before its expansion. It would appear then, that a satisfactory explanation of this increase of combustible matter has not yet been given.

G. H.

VII.

A new Process for claying Sugars, proposed by CIT. HAPEL-LACHENAIE, *Chief Apothecary of the Military Hospitals of Guadaloupe, to the Agents of the Consuls of the French Republic in the Windward Islands* *.

BEING called upon by the scientific mission confided to me by the Government, to employ myself in every kind of research which may prove interesting to the cultivation and productions of this colony, I have thought it my duty to endeavour to discover a simple, easy, and cheap method of dispensing with the pottery which is used in the fabrication of clayed sugars. Introduction.

There was even an urgent necessity that I should direct my attention to these objects, because most of the proprietors of these colonies, being at this moment in want of the forms and pots hitherto considered as indispensibly necessary for the claying sugars, are obliged to wait till the small number of pots they possess shall be cleared of the sugar they contain before they can fabricate more. Want of Pans for claying sugars in St. Domingo,

The delays occasioned by this inconvenience are the more prejudicial to the inhabitants in general, because they are obliged, on this account, to defer cutting their canes when at the most advantageous time of their growth; and as they cannot perform this at the most favourable season, their losses are incalculable by suffering the canes to grow old, and finishing obviously productive of great loss.

* From the Annales de Chimie, XL. 73.

their

their gathering in the ſeaſon when they afford very little ſugar, in compariſon with what they would have yielded if they had been cut in time.

While this retardation diminiſhes the immediate product, it has an influence on the vigour of the ſucceeding ſhoots, as I have obſerved, and renders their advancement leſs ſpeedy. And by this means it expoſes the farmer of national plantations to fail in payment of the terms of their location.

Experiments for a new proceſs,

I ſhall not dwell upon the various experiments I made before my ſucceſs was complete, but ſhall confine myſelf to thoſe means which have beſt ſucceeded with me, and to which I have confined myſelf in the ſubſequent practice in my ſugar-work.

for diſpoſing the ſugar to be clayed in large veſſels.

This proceſs conſiſts in diſpoſing the ſugar in receptacles of 12,937 cubic feet, each containing 26 ordinary forms, and to clay it in theſe ſame receptacles, of which I ſhall proceed to give a deſcription :

Firſt uſed by Citizens Boucherie at Paris,

The priority of the method which I propoſe belongs to Cits. Boucherie, who firſt adopted it in their refinery of Bercy near Paris. They were, as far as I know, the firſt who conſtructed veſſels to clay the raw ſugars they received from our colonies previous to their refining them. But though this invention is undoubtedly theirs, I alſo have a claim as to the degree of improvement which their veſſel did not poſſeſs, but which I have given them by rendering them more convenient, advantageous, and economical *.

and improved by the Author.

Deſcription of the apparatus of Citizens Boucherie.

The veſſels of Citizens Boucherie were, as nearly as I can recollect, about 15 or 18 inches deep, and five feet wide. They were ſquare, and the bottom of each was perforated with a great number of ſmall holes for the diſcharge of the ſyrup, which fell into a ſecond veſſel leſs deep, but of the ſame dimenſions as the others as to their width. This ſecond veſſel was lined with thin metallic plates, of an alloy invented by Mr. Hiſkerdeau, a Spaniſh chemiſt. The upper veſſels

* I attended their operations in the year 1784, before I came to this colony. I had alſo the advantage at that time of giving ſeveral lectures in their eſtabliſhment to ſome cultivators of St. Domingo and other ſugar iſlands, to facilitate their knowledge of the doctrine and proceſſes of that intereſting manufacture which they frequently viſited.

were

were supported on their edges by a level and firm framing, the inferior vessels stood on the floor.

The upper vessels were filled with raw sugar intended to be clayed, which was first well divided or crumbled in order to its equal distribution. This was levelled and compressed as equally as possible, to form what we commonly call, the bottom *(les fonds)*. Upon this mass the diluted and prepared earth, properly adapted to the operation, was very carefully poured.

Charged with raw sugar.

Though this process may appear very simple at first sight, it nevertheless presents difficulties which could scarcely be overcome in that manufactory, and which would be almost impossible to surmount in ours, where the men employed in this business have neither the understanding nor the skill of those of Citizens Boucherie; and it was not till they had practised for some time that they became able to perform it without difficulty.

This simple process is liable to objections.

The first operation in which a failure may be made for want of skill, is that of levelling with a trowel an elastic surface of 25 square feet. The second is to compress equally that surface in order to give solidity to the bottom, that the diluted earth may extend to the same thickness on all sides, and find that surface so close as to admit the penetration of the water only.

It is difficult to level and press the sugar in the large vessels.

All those who are engaged in the manufacture of sugar must be aware, that if any inequality exists in the levelling of the bottom, the water which gradually leaves the earth naturally flows to the lowest place, where, if the compression be not every where the same, the water insinuates itself into the most porous part; and in either of these cases, this fluid being conducted with the earth towards a single point, is collected in sufficient abundance to dissolve the sugar at that place, and form what is called a fox (*un renard.*) When this happens the operation of claying fails, for it is known that the portion of earth retained at the surface acts little upon the rest of the mass of sugar. This inconvenience, which occasions a great loss in the product, is also found to change its quality, which is worse on this account.

Mischievous consequences of bad levelling or pressing.

The water flows to the lowest place and dissolves the sugar; while the rest is not full purified.

In the construction of the cases or vessels of Citizens Boucherie, which cannot be removed, other inconveniencies are found which do not exist in mine. One of these is the difficulty

The sugar is not easily taken out.

culty of taking out the fugar after its drainage. A fecond is, that the holes in their bottom are too fmall, which renders them liable to be obftructed, and the procefs to be greatly retarded.

(To be concluded in our next.)

VIII.

Defcription of the Cryftalline Forms of the Anhydrous Sulphate of Lime, with fome Obfervations on this Subftance. By M. LE COMTE DE BOURNON, *Member of the Royal and Linnæan Societies of London. Tranflated from the Original, communicated by the Author *.*

Sulphate of lime containing no water is a new object in mineralogy.

GYPSUM, or the fulphate of lime deprived of water, is a new object in mineralogy: fcarcely has the fcience as yet caft a glance upon this fubftance, and none of its cryftalline forms have hitherto been determined. Having had opportunities of examining fome of thefe forms, and at the fame time of comparing a number of fpecimens of this fubftance, procured from different countries, I felt myfelf induced to lay before the Public my obfervations upon this fubject, thinking they might contribute to throw fome light upon the nature of feveral ftones, with which we are not yet perfectly acquainted.

Its primitive form is a right prifm with rectangular bafes.

The primitive form of this fubftance, which has been defignated by the name of Anhydrous Sulphate of Lime by the Abbè Hauy, is a rectangular paralelopiped, as had been prefumed by this able mineralogift; but it does not appear that this paralelopiped can be the perfect cube, as is indicated by the habitual form of the cryftals, which when moft fimple, is always a right prifm with rectangular bafes, having two oppofite fides broader than the two others (Fig. 1, Plate XI.), and feems equally to be indicated by the circumftances which accompany this form. The mechanical divifion is very eafy upon all the faces of this cryftal; but the longitudinal ftraight faces prefent, in this refpect, a little more refiftance than the others.

Eafily divided.

* And afterwards revifed by him.

The

The broad faces, as well as those which terminate this rectangular prism, have generally a very brilliant lustre; the longitudinal narrow ones faces are duller, and very frequently striated in the direction of their length. The broad sides are besides characterised by a pearly reflection, similar to that which is peculiar to the zeolite stilbite, and this brilliancy remains even after those faces have been divided: they also habitually exhibit the intersection (*(entrecroissement)* at right angles of the joints belonging to the other faces. **Lustre.**

This crystal presents, along its longitudinal edges, a decrease which replaces each of these sides by a plane unequally inclined upon the adjacent faces: it forms with the broad sides of the prism an angle of 130°, and of 140° with the narrow sides, (Fig. 2.) The prism is therefore octahedral, and has four edges of 130°, and four others of 140°. **Secondary forms. Octahedral prism.**

These new planes frequently join each other, upon the narrow sides of the prism, and they then convert the octahedral into a hexahedral form, having four edges of 130°, and the two others of 100° (Fig. 3.) The pearly sides frequently then continue to be the broadest; in other instances, the six sides of the prism are equal, or nearly equal with each other. I have not yet seen any secondary faces placed upon the terminal edges. **Hexahedral prism.**

The crystals upon which this description is founded are of considerable magnitude: some are more than an inch in length. They are of a fine flesh colour, and formed part of one of the specimens brought from the Tyrol, in which the anhydrous sulphate of lime is mixed with muriate of soda or common salt; but as this salt is intirely foreign to it, and only interposed between its parts, frequently even in a manner that is very perceptible to the eye, the form of these crystals ought to be considered as being really that of the pure anhydrous sulphate of lime. Besides, I have seen in Mr. Greville's cabinet at London, a specimen in which this substance presents exactly the same aspect, and the same colour, and which at the same time is totally destitute of salt. In this, the anhydrous sulphate of lime is confusedly intermingled with actinote of a pale green colour, with some parts of cupreous pyrites, and of the black and very magnetic oxide of iron. The locality of this interesting specimen has not been preserved; but there is every reason to presume that it came from Sweden. **Foreign admixture of muriate of soda.**

In

Groups of cryſtals;

In the Tyroleſe ſpecimens, ſimilar to that which afforded the cryſtals which I have juſt deſcribed, theſe are for the moſt part ſtrongly engaged and applied againſt each other, croſſing each other in different directions; ſome of them, however, are inſulated. But as many of them are united by their broadiſh or pearly faces, they have no very ſtrong adheſion, and it is always ſufficiently eaſy to ſeparate them with the edge of a knife. Theſe ſpecimens frequently exhibit in their maſs more or leſs conſiderable portions of pure common ſalt; ſeveral of their cryſtals are conſiderably tranſparent.

eaſily ſeparated.

Peculiarity. The preſence of ſulphur of antimony and quartz.

A piece of this ſubſtance which I procured for Mr. Chenevix, for the purpoſe of analyſing it, exhibited a peculiarity which deſerves to be remarked. On breaking ſome of its cryſtals, ſmall needles of ſulphuret of antimony were perceived within it, adhering for the moſt part to ſmall groups of cryſtals of quartz. Not a trace of either of theſe two ſubſtances was found in the other parts that were ſubjected to analyſis; the ſame was the caſe with the carbonate of lime, which Mr. Klaproth has indicated at $\frac{11}{100}$ of the analyſis which he made of it, and which, undoubtedly, was likewiſe only an extraneous or interpoſed ſubſtance.

Quartz in another ſpecimen.

In like manner, in the ſtone of Vulpino, obſerved and deſcribed very accurately and carefully by Meſſrs. Fleurieu and Bellevue, in the 2d Vol. of the Journal de Phyſique for 1798, the anhydrous ſulphate of lime is mixed with interpoſed particles of quartz, which, according to the analyſis made of it by Mr. Vauquelin, are in the proportion of $\frac{16}{100}$ to its whole maſs. I am indebted to the friendſhip of Mr. Fleurieu for two ſpecimens, which preſent two diſtinct varieties of this intereſting ſtone.

One of them is of a good blue colour, is partly of a very fine ſandy grain, and partly coarſer and lamellated: it greatly reſembles the carbonate of lime known by the name of ſaline marble. An immenſe number of ſmall laminæ are obſervable in it, which croſs each other in different directions, and are found by the magnifier to be perfectly rectangular.

The other is of a darker aſh coloured grey; it is leſs pure than the former: ſome portions of a true gypſeous earth are obſerved in it, containing a ſmall quantity of argill and of carbonate of lime. Its ſubſtance is more compact than that of the

the preceding ſpecimen; its laminæ are larger, their rectangular form is more perceptible, and ſeveral of them have a pearly reflection.

Of two other pieces of the ſame ſubſtance, in the cabinet of Mr. Greville at London, which appear to me to belong to the anhydral ſulphate of lime of the ſalt-works in the canton of Bern, and both which are perfectly pure, the one is white with a little of a blueiſh caſt, the other has the ſame blueiſh tinge, but deeper, and greatly reſembles in colour the pale blue ſapphire, known by the name of water ſapphire. Both have a coarſe granulated texture, and are compoſed of a combination of rectangular laminæ, which croſs each other in different directions, as may eaſily be diſcerned with the naked eye. But the laminæ of that in which the blueiſh colour is the moſt intenſe, preſent beſides ſo lively a pearly reflection, that it might eaſily, at firſt ſight, be miſtaken for a maſs of zeolite ſtilbite. Rectangular laminæ, of ſome thickneſs, may be ſeparated from it, which, like the cryſtals before deſcribed, are eaſily divided in every direction. Otherſpecimens.

The ſpecific gravity of the anhydrous ſulphate of lime, mixed with common ſalt, proved to be 2940. That of the ſpecimens which I mentioned in Mr. Greville's cabinet, namely, that in which the laminæ had the ſtrong pearly reflection, was 2957, and the other 2929. Of the two of Vulpino, the moſt compact was 2951, and the other 2933. Specific gravity.

The hardneſs of this ſubſtance, in all the ſpecimens, is ſomewhat ſuperior to that of the carbonate of lime; in all of them alſo the anhydrous ſulphate of lime is fuſible by the blow-pipe without ebullition, and affords an opaque glaſs. Hardneſs.

There is a marked difference between them, with reſpect to the phoſphoreſcence, upon an heated ſhovel. The anhydrous ſulphate of lime of Vulpino, gives a pretty ſtrong orange-coloured light; that mixed with common ſalt, affords a very faint bluiſh light; that of the ſalt-works of Bern, none at all; and laſtly, that which I mentioned as containing actinote, with attractive oxide of iron, &c. gives a light ſomewhat more reddiſh than that of Vulpino. Phoſphoreſcence.

Gypſum deprived of water, or anhydrous ſulphate of lime, therefore, poſſeſſes characters altogether different from thoſe of the ſulphate of lime which contains that liquid; and as in theſe two ſtones the ſulphuric acid and the lime exiſt in the ſame The abſence of water renders the properties altogether different.

ſame proportions, it cannot be doubted but that the preſence of the water totally changes the nature of the combination of acid and earth when it comes to be joined with it, which, it appears to me, can only take place ſo far as this water becomes itſelf an eſſential component part of the ſtone.

Remarks in proof that water is eſſential to the compoſition of many ſubſtances.

This is not the only ſubſtance which enables us to aſcertain that the water, which formerly was conſidered as a part foreign to the ſtones which contained it, becomes in reality, in ſeveral of them, an ingredient eſſential to their nature. The analyſis which Mr. Chenevix has made of the different ſpecies of arſeniates of copper, have preſented us a ſtriking example of this, eſpecially in that variety of the third ſpecies of my deſcription (ſee Philoſophical Tranſactions, 1801) to which I have given the name of Hematiform. A movement of decompoſition, which is confined to the gradual loſs of their conſtituent water, totally changes the colour of theſe arſeniates, and at laſt completely diſcolours them, at the ſame time that it renders thoſe that before had ſome feeble tranſparency, perfectly opaque. This loſs of the water always commences at the exterior, and in this caſe the interior part preſerves all its tranſparency as well as its colour, whilſt the exterior part is diſcoloured, and exhibits by the ſhrinking of its ſurface, ſometimes to a very conſiderable degree, ſenſible marks of the loſs which it has ſuſtained; the water amounting to about one-fifth of its maſs *.

Arſeniates of copper.

* In my deſcription of the arſeniates of copper quoted above, I have conſidered this hematiform arſeniate, as well as thoſe which I have deſignated by the terms of indeterminate, capillary, and amianthiform, only as being varieties of the ſpecies in the acute octahedral form: the copper and the arſenical acid are in fact contained in them in the ſame proportions; but the water, which adds a new conſtituent part to them, and which did not exiſt in thoſe varieties of this third ſpecies, which are in perfectly determined cryſtals, forms with them a real hydrate of this third ſpecies. I therefore think that it would be proper to ſeparate theſe varieties, in order to form with them a fifth ſpecies perfectly diſtinct from the third. The arſeniate of copper is one of the moſt aſtoniſhing productions of the mineral kingdom, by the immenſity of the aſpects under which it preſents itſelf, all which, nevertheleſs, have certainly a particular cauſe, which I am very far from pretending to have aſcertained.

In

In the ſame manner it is, that, in the blue carbonate of copper, which, according to the inveſtigations of Mr. Prouſt, appears to contain a conſiderable quantity of water, far ſuperior to that which exiſts in the green carbonate, the cryſtals ſeem to paſs, at their ſurface, into the ſtate of green carbonate, by the mere loſs of a certain portion of their conſtituent water. This ſpecies of decompoſition is ſometimes even ſo conſiderable in them, that it exiſts, for example, amongſt the ſpecimens in large and ſuperb cryſtals which come from Siberia, cryſtals, of which the form having undergone no change, belongs to the blue carbonate, but which have entirely paſſed, throughout their whole ſubſtance, into the ſtate of green carbonate.

Blue carbonate of copper.

I am fully perſuaded, that when water is once conſidered and admittted as a conſtituent part in the compoſition of numerous mineral ſubſtances, it will ſoon be acknowledged to contribute greatly, by its preſence or its abſence, to the difference which ſubſiſts between ſeveral ſtones: thus I am ſtrongly inclined to believe that the carbonate of lime of ſlow ſolution, the hardneſs and ſpecific gravity of which are ſo much ſuperior to the ſame characters in the ordinary carbonate of lime, differs from this latter perhaps only by a difference in the water of compoſition. Moſt certainly this difference cannot proceed either from the preſence of argil, or from that of magneſia. I know ſome dolomies which certainly do not contain any trace of argil, whilſt, at the ſame time, I know carbonates of lime, which are highly charged with magneſia, and which, nevertheleſs, are very ſpeedily diſſolved in the acids: Of the number of theſe latter are, for example, moſt of the calcareous ſpars with a pearly reflection and greaſy aſpect.

Water is of great importance as an ingredient which modifies the properties of bodies.

I am much inclined alſo to think, that the ſpecies of Chalcedony, named cacholong, may owe its difference from the common chalcedony only to the circumſtance, that the latter contains water of compoſition of which the chacolong is deſtitute; and that the tranſition of the chalcedony into this ſtate, and ſubſequently into the hydrophanes, depends, in a great meaſure, upon the loſs of this water. The probability of this opinion remains to be ſettled by future obſervations.

Cacholong probably owes its peculiar properties to the abſence of water.

IX.

Analyſis of Natural and Artificial Anhydrous Sulphate of Lime, by RICH. CHENEVIX, *Eſq. F. R. S. M. R. I. A. Communicated by the Author.*

Component parts of common ſulphate of lime.

THE proportion of the elements in common ſulphate of lime, ſuch as I have ſtated them in the Tranſactions of the Royal Iriſh Academy, are ſomewhat different from thoſe given by former chemiſts. Monſ. Fourcroy, in his "*Syſtême des Connoiſſances Chimiques,*" and in his "*Synoptic Tables,*" has determined them in the following order: ſulphuric acid 45, lime 32, water 22. If we deduct the quantity of water, and reduce the remainder to the quintal, we ſhall have the following proportions: ſulphuric acid 58,5, lime 41,5. As the object of the preſent Paper is merely the Anhydrous ſulphate, I ſhall not take further notice of the quantity of water that may be contained in common ſulphate of lime, whether natural or artificial.

Lime diſſolved in muriatic acid and ſulphuric acid added, and the whole expoſed to violent ignition, left dry ſulphate of lime containing 56,3 lime, and 43,6 acid.

I took one hundred parts of lime, prepared with the greateſt care, and diſſolved them in muriatic acid. I then poured ſulphuric acid into the ſolution, and heated the whole in a platina crucible, at firſt gently, but afterwards to violent ignition. The augmentation of weight in the lime and the crucible, (which had been weighed before the operation) amounted to 78,5, and was combined ſulphuric acid. This experiment therefore indicates, that ſtrongly calcined ſulphate of lime is compoſed of lime 56,3, and 43,6 of ſulphuric acid.

Sulphate of barytes contains 24 acid in the 100 parts.

I then took one hundred parts of calcined ſulphate of lime, and decompoſed them by oxalic acid, to render them more ſoluble, and then diſſolved them in muriatic acid.

I precipitated the ſolution by muriate of barytes, and obtained 182 of ſulphate of barytes. Hence it is evident that 182 of ſulphate of barytes, and 100 of calcined ſulphate of lime contain the ſame quantity of ſulphuric acid = 43,6; which proportion gives 24 as the quantity of ſulphuric acid, in 100 of ſulphate of barytes. This quantity is nearly intermediate between that given by Thenard, and that which I had already ſtated. By this experiment I eſtabliſhed a ſtandard, to which I might refer every kind of ſulphate of lime.

 It

It is evident that calcined ſulphate of lime, or artificial anhydrous ſulphate of lime, contains 43,6 of ſulphuric acid. It remains to prove its chemical identity, with the natural anhydrous ſulphate.

I took one hundred parts of this ſubſtance, in as pure a ſtate as the Count de Bournon could procure it, and ſubmitted it to the ſame experiments. I obtained from this experiment 187 of ſulphate of barytes, which announces 44,88. I ſhall never expect more uniform reſults in two analyſes, even of the ſame ſubſtance, and do not heſitate to pronounce, that the two ſulphates are, chemically ſpeaking, one and the ſame thing. The natural anhydrous ſulphate of lime was treated like the artificial, and gave 44,88 acid.

The Abbé Hauy in his late work has given the proportions in anhydrous ſulphate of lime, according to an analyſis of Monſ. Vauquelin. This excellent chemiſt found its elements to be nearly in the inverſe order of my ſtatement, and ſuch as Fourcroy has eſtabliſhed them for the artificial ſulphate, deducting the water of cryſtallization. I ſhould ſtill have doubted the accuracy of my experiments, if I had not diſcovered a cauſe that may explain the difference which exiſts between his reſults and mine. Vauquelin's experiments gave much more acid;

The French chemiſts have mentioned two varieties of ſulphate of barytes, one of which contains 13 per cent. of ſulphuric acid, and the other 33. If therefore we eſtimate the quantity of acid contained in ſulphate of lime, by the ſulphate of barytes containing 33, and not by that containing 24 per cent. we ſhall have a much greater quantity of ſulphuric acid, as a conſtituent part of ſulphate of lime. moſt probably from his aſſuming a higher proportion of acid in the ſulphate of barytes.

Among the ſpecimens which the Count de Bournon gave me for trial, there were ſome which contained muriate of ſoda. This ſalt was eaſily extracted by water alone; and the proportion of it differed in different ſpecimens. Klaproth had ſome ſpecimens in which he found carbonate of lime, and even ſilica. But as I have examined ſome in which I can poſitively aſſert that there was neither the one nor the other, thoſe ſubſtances may be looked upon as merely accidental. Some ſpecimens of the anhydrous ſulphate contained muriate of ſoda; but it was accidental.

X. *Abridgment*

X.

Abridgment of a Memoir of Mr. PROUST *on Tanin and its Species* *.

Uncertainty of the procefs of feparating tanin by muriate of tin.

THE procefs which Mr. Prouft has given for feparating Tanin by muriate of tin is fubject to confiderable uncertainty when it is ufed, as an Englifh Chemift has lately done to fix the proportion of this principle in vegetable juices. He thinks, therefore, that it may be of utility to announce thefe caufes of error, in order that philofophers, who are engaged in this department of refearch, may place lefs confidence in it, and turn their thoughts to fome more perfect method. To thefe remarks he adds an account of certain varieties which he thinks he has perceived in the genus of Tanin.

When the oxide of tin is feized by the tanin, the muriatic acid inftead of being fet at liberty diffolves part of the tannate.

1. When this muriate is faturated with an aftringent juice, it happens that the muriatic acid takes up in folution a portion of the tannate of tin, fo that what is collected on the filter reprefents, in truth, only a part of the tanin principle contained in the plant. This effect is fimilar to what happens in the preparation of ink, the black dye, and in every cafe where a dyeing principle can deprive an acid of the oxide which it held in folution. The affinity oft his acid not being capable of remaining inactive exerts itfelf upon the coloured oxide.

This tannate may be thrown down by careful additionof alkali.

When fmall dofes of alkali are added to the liquor the refidue of the tannate may be made to precipitate without even touching the gallic acid if it be prefent; and if this point be exceeded it may foon be perceived by the green colour which the fluid receives from contact of the air. In that cafe a few drops of acid will be fufficient to difpel the cloud by feizing the excefs of alkali and fetting the gallic acid at liberty. But as, on the other part, it is neceffary at firft to employ an excefs of muriate to affure the precipitation of the whole of the tanin, there is danger of loading the tannate with a certain excefs of oxide of tin.

But the tannate will be overcharged with oxide if too much of the muriate has been ufed.

The muriate precipitates extractive matter and perhapsother principles along with the tanin.

2. The prefence of tanin does not exclude the extractive principle in the juice of a plant, and as the muriate precipitates this laft as well as the former, the extractive principle will therefore become a new fource of miftake in the eftimate of the

* From the Annales de Chemie XLII. 89.

the quantity of tanin. It may alſo happen that the vegetable juices may contain many other ſubſtances capable of decompoſing the muriate either directly or indirectly, ſo that this re-agent can never be depended upon with confidence.

There are beſides earthy ſalts in theſe juices as Vauquelin has ſhewn. Some, as for example Sumac contain ſulphate of lime in abundance. If therefore we uſe the alkali to complete the ſeparation of the tannate, the precipitate will alſo be charged with an earthy depoſition.

When earthy ſalts exiſt in the vegetable, theſe will let fall their baſes when the alkali is added.

3. On reflecting upon the means of avoiding the errors cauſed by the muriatic acid, Mr. Prouſt tried a proceſs which he had ſucceſsfully uſed to ſeparate the colouring principle from the gelatinous mucilage which accompanies it in cochineal. It conſiſts in heating, or even ſimply agitating, the aſtringent juice with the oxide of tin prepared by nitric acid, and kept under water. The oxide in fact, becomes loaded with tanin in a few days. But if the juice of a plant which is not aſtringent, or a diluted extract, be treated in the ſame manner, it will alſo become deprived of the whole of its extractive matter: and the gum and the ſalt will remain alone in the fluid, This method cannot therefore lead us to our object.

If the pure maximum oxide of tin be heated or agitated with tanin a combination takes place and, mucilage, if preſent, is left:

but this proceſs will not ſeparate the extractive matter.

4. What appears moſt ſurpriſing to this chemiſt in the preſent experiment is the deſtruction of the gallic acid, or probably its tranſition into a ſtate in which it cannot perform the functions of that acid. The fluid in fact when cleared of the tannated oxide by the filter has no longer either colour or taſte, and makes not the ſlighteſt impreſſion on the ſolutions of iron, nor even upon turnſol. When examined by every trial the fluid is found to be mere water.

Singular event in the laſt experiment. The gallic acid is deſtroyed.

5. Suſpecting, nevertheleſs, that this acid might be combined with the oxide of tin, he paſſed the tannate of the laſt deſcribed proceſs into potaſh. The product was a coloured fluid, in which he found no ſign of the gallic acid: for it did not aſſume by expoſure to the air that green ſhade which it always indicates when ſaturated with an alkali. He precipitated the tanin by a diluted acid, a portion remaining in ſolution, as happens in this caſe and he proceeded to examine that which remained upon the filter; but he ſoon perceived that it had alſo advanced towards a ſtate in which, as we ſhall ſhew, he found infinitely leſs tanin than before.

When the tanate &c. was decompoſed by potaſh no gallic acid was found and much leſs of tanin.

6 Boiling

The tanin that is separated is not totally soluble in boiling water; neither does it precipitate glue, nor has it the taste or smell of tanin &c. It resembles extracts.

6. Boiling water cannot totally dissolve it. Its solution no longer precipitates glue; it has neither the harsh taste nor the odour of tanin. With the red sulphate of iron it affords only a whitish grey precipitate, and lastly it does not afford a magma with the muriate of tin; it is precipitated merely in the manner of extracts, to the taste of which it in some measure approaches though it does not possess their bitterness. These are the alterations to which the taning principle is subject when combined as before mentioned with tin oxided to the maximum.

It was suspected that the tanin had taken oxigen from the oxide; but experiments on the latter did not seem to indicate this loss.

7. From these changes he suspected that the tin might have yielded to it that portion of oxigen which constitutes the difference between the oxide at the maximum and the oxide at the minimum, as happens with the oxide of iron in ink, hermetically closed. In order to ascertain this he dissolved in muriatic acid the oxide what had been deprived of tanin by potash; but he discovered no indication of that kind. The solution produced no change in that of gold, nor in corrosive sublimate. It was at the maximum. It is true that with respect to the oxide of tin at the minimum, as well as that of iron, washing and exposing to the air speedily bring it to the maximum.

Theory of this conversion yet unknown.

Whether by oxidation or by whatever other process, the tanin principle did at length pass to the state of ordinary extract. Tanin precipitates glue, but extract does not; this is the difference between them, The influence of some affinity, which the author has not sufficiently developed, must have changed its radicals in their primordial arrangements or in their proportions; and it may be supposed that the gallic acid likewise after having been subjected to these changes became assimilated in the same state and by the same causes.

Concerning the Varieties of Tanin.

The genus *tanin* composed of species.

If in the series of immediate principles which compose the whole of vegetable matter we consider the tanin of galls as a genus, because this in fact possesses the qualities in the highest degree, it is easy o form a notion that this genus may have its species, and may, as well as sugar, gum, starch, &c. affect different modifications. There are various species of sugar, resin, gum &c, and there may be also various species of tanin. Mr. Proust thinks that he has found this to be the case.

Cachou

Cachou, or Terra japonica.

Cachou is an aftringent: it is foluble in alcohol and in water. It precipitates glue abundantly and forms with it a vinous (q.) magma which has neither the confiftence nor the infolubility of the tannate of galls. Qualities of the various fpecies of tanin. Cachou or Terra japonica.

It reduces muriate of gold, it precipitates the muriate of tin and affords a violet coloured ink with red fulphate of iron. It is a tanin *fui generis*. It dyes filk.

Dragons Blood.

That which is pure and comes to us in calebafhes is foluble both in water and in alcohol; its tafte is harfh; it dyes filk of a bad wine colour. It abundantly precipitates glue, the muriate of tin, the red fulphate, and difoxides gold; it is alfo a fpecies of tanin. Dragon's Blood.

Sumach.

The tanin which this fubftance contains, abundantly precipitates glue, and affords a white magma without confiftence. Like tanin it is feparated from the decoction of fumach by the carbonate of potafh; its curdled depofition is again foluble in hot water with the exception of a fmall quantity of chalk. Sumach.

Barites and the oxalic acid demonftrate the exiftence of abundance of lime and fulphuric acid; but it will be neceffary to afcertain by experiments on the green fumach whether fulphate of lime be one of its principles, or an adulteration. The author is not furprifed at finding it, fince he obtained this falt in confiderable quantity from the juice of cabbage, and of folanum lycoperficon, which is cultivated in the gardens under the name of lomates. This juice alfo contains gallic acid; it becomes green in the air when faturated with potafh. It reduces gold, decompofes muriate of tin and the red fulphate, with which it affords a thick ink.

Yellow Wood, or Fuftic.

It contains a fpecies of tanin. Like tanin it precipitates the folution of glue; a folution of falt is fufficient to precipitate it. It is foluble in water and in alcohol. It reduces gold, decompofes muriate of tin and the red fulphate, by means of which it dyes filk of a greyifh yellow. Yellow wood or fuftic.

Fuſtet.

Fuſtet.

It is a pure dyeing extract ſoluble in water as well as in alcohol. It contains a ſmall quantity of gallic acid, but does not change the ſolution of glue. It reduces gold, precipitates the metallic ſalts, and has no gummy portion.

Grains of Avignon, or French Berries.

Gr. d'Avignon, or French berries.

They afford a dyeing extract of the ſame nature, without gum or tanin. It reduces gold, &c.

Brazil Wood,

Brazil wood.

Affords alſo a dyeing extract ſoluble in alcohol, without tanin, or gum, reducing gold and precipitating the metallic ſalts.

Reduction of gold is no excluſive character of tanin.

Mr. Prouſt has remarked, that the reduction of gold has ceaſed to be a characteriſtic quality, ſince he obſerved the muriatic ſolution abandon this metal to all the tinging ſubſtances, ſuch as aniſe, cochineal, gum guttæ, gallic acid, verjuice, wine, vinegar, the juice of all fruits, manna, gum, and ſugar, though ſomewhat ſlowly.

Concluding obſervations.

The author concludes by obſerving, that tanin has its varieties like the other immediate products; that the property of precipitating glue is the generic indication, by which they are diſtinguiſhed from extracts, which do not alter that ſubſtance; and laſtly, that the different ſpecies of tanin, particularly thoſe which have been diſcovered in the barks of trees, cannot be compared together as to their force and their uſeful qualities, but by obſervations upon ſkins which have been ſubmitted to their action.

Taning matters *require to be tried by actual preparation of leather.*

Note. Plaſter muſt be infinitely common in vegetables. Mr. Prouſt has found it in verjuice, grapes, apples, gooſeberries, &c.

Sulphate of lime very common in vegetables.

B. L.

XI. *Deſcription*

XI.

Description of an Apparatus for heating Water by waste Steam. Invented by Mr. ARTHUR WOOLF.

THE following apparatus was erected at the extensive brewery of Messrs. Meux and Co. in August, 1800, and has been in use ever since. I saw it work a few weeks ago, and observed with great pleasure the facility and precision with which it operates, and I have great satisfaction in presenting it to the reader as a very judicious and useful combination. Engine for heating water by steam.

Plate IX. A represents a steam pipe from the brewing copper. Particular description.

B a valve with its weight.

C the vessel in which the steam is condensed.

D a pipe that conveys the cold water from a reservoir.

E a conical valve through which the water is injected. It is connected with the lever F.

G is a bended pipe to prevent any of the steam from escaping with the hot water.

H a small receiver from which the hot water may be conveyed to different situations by means of pipes and cocks.

I a pipe open to the receiver to prevent a vacuum in case the water should be made to descend in any of the pipes.

K a small pipe to convey the steam into the regulator.

L the regulator which is composed of three cylinders, the outside and inside being closed together at bottom, leaving a cavity between, which is filled with water; the middle or moving cylinder is inverted and close at top. It serves for a piston, and is connected to the lever M, on which is a sliding weight N, by which the quantity and heat of the water may be varied at pleasure.

O is a valve through which the steam is let out when not used for heating water.

The effect of this engine may be easily understood. The weight of the inverted hollow piston L presses down the valve E by means of the levers, and this pressure may be regulated by fixing the weight N nearer or farther from the centre of the upper lever. When the steam through A has acquired a certain degree of strength in the vessel C, it raises the piston by its action through K, and consequently opens the valve E. A sheet of water immediately dashes throngh, as represented Explanation of the manner in which it acts.

3 in

in the figure, and by condenfing the fteam, fuffers L again to defcend; and, after a vibration or two, the effect of the fteam to raife the pifton and of the injection to deprefs it, balance each other, fo that the levers remain nearly motionlefs. It is evident that the injection will be lefs, the fteam ftronger, and the water which paffes off through G hotter the nearer the weight M is to the centre of motion. And in this refpect the apparatus is fo effectual that the water may be heated to 210 degrees, and the quantity that paffes off is from 100 to 180 barrels per hour, according to the temperature, as governed by the pofition of the weight M.

XII.

Defcription of an improved Drawback Lock for Houfe Doors. By Mr. Wm. Bullock. *From the Tranfactions of the Society of Arts, who adjudged a Reward of Fifteen Guineas to the Inventor.*

Copy of a Letter to the Secretary of the Society Mr. Charles Taylor.

SIR,

Inconveniences and danger of the common drawback lock.

I HAVE herewith fent, for the infpection of the Society, an improved Drawback Lock for Houfe Doors, &c. which improvement is in latching the door; for it is well known, particularly in damp weather, that the air drawing through it, rufts the head or bevel of the bolt, by which means it requires great force to fhut the door, and occafions a difagreeable noife, befides fhaking the building.

It has frequently happened that the houfe has been expofed to robbery from the door being left unlatched, when fuppofed to be faft. This improvement removes all thofe inconveniences, as it lets the bolt fhoot into the ftaple immediately when the door clofes, but not before; and the reliever works fo very eafy, that the door is made faft with one twenty-fourth part of the force required with locks upon the common conftruction.

By

By an experiment with the lock ſent herewith, it will be proved that two ounces added to the reliever, wiil ſhoot the lock with more eaſe than three pounds will do, applied to the bevel bolt; and if the lock is ruſty, the advantage will be much more in favour of the new method. I flatter myſelf it will be of great utility to the public, as its conſtruction is ſimple and cheap. It may be added to any old lock, as may be ſeen from that now ſent. It may be advantageouſly applied to French windows and glaſs doors, as it prevents the door from being ſtrained, or the glaſs broke, by the force applied to ſhut them. I have fixed ſeveral locks, upon this new principle, which anſwer well; and if the invention meets with the approbation of the Society, I hope to be rewarded according to its merit. Advantages of the improved lock.

I remain, with reſpect,

SIR,

Your moſt obedient Servant,

WILLIAM BULLOCK.

No. 6, *Portland Street, Soho, May* 5, 1801.

Plate X. Fig. 2. A. Is the new iron latch here affixed to an old common drawback houſe lock. Deſcription.

B. An iron pin at one end of the latch, on which pin it is moveable.

C. A projecting part of the latch, which, when the common ſpring bolt D of the lock is drawn back, in the uſual manner, is forced into the nick on its higher part at E, by the ſpring F, underneath the latch.

The bolt D then remains within the lock, until, on cloſing the door, the reliever G gently preſſes on the lock box, fixed in the common way on the door cheek; which preſſure draws the projecting part C out of the nick E, and permits the end of the bolt D, by the force of the ſpring G, to ſlide into the lock box, and faſten the door.

XIII. *Deſcription*

XIII.

Description of an improved Mill for grinding hard Substances. By Mr. GARNETT TERRY. *From the Transactions of the Society of Arts, who adjudged the Silver Medal to the Inventor.*

Description of an improved mill.

MR. Terry, whose residence is No. 20, City Road, Finsbury Square, has constructed this mill on a large scale, and there is also a model deposited in the Society's collection.

Plate X. Fig. 1. A. The hopper, or receptacle of the articles which are intended to be ground.

B. A spiral wire, in the form of a reversed cone, to regulate the delivery of them.

C. An inclined iron plate, hung upon a pin on its higher end: the lower end rests on the grooved axis D, and agitates the wire B.

D. The grooved axis, or grinding cylinder, which acts against the channelled iron plate E.

F. A screw on the side of the mill, by means of which the iron plate E is brought nearer to or removed further from the axis D, according as the article is wanted finer or coarser.

G. The handle, by which motion is given to the axis.

H. The tube from whence the articles, when ground, are received.

*** The front of the mill is taken off, in order to show its interior construction.

XIV.

Remarks on Dr. Thomson's Theory of Combustion. By C. R.

(Received June 15, 1802.)

General remarks on Dr. Thomson's theory, and the distinction between combustion and oxigenation.

THE scientific world are so highly indebted to Dr. Thomson for many original communications, and for the very perspicuous manner in which he has explained many of the phenomena of chemistry, that every thing that is presented to the world under the sanction of his name, is intitled to much consideration: if in some instances we are induced to hesitate in the yielding of our assent, we cannot but do justice to the ingenuity of his reasonings, and at the same time acknowledge the

the very luminous manner in which he conveys information, on every ſubject that he treats. The Paper under conſideration is particularly an inſtance in point, and if we cannot go the full length with the author, we muſt at leaſt acknowledge, that in the chief, his diſtinctions are accurate, and his reaſoning juſt. Nothing can be more evident than the difference which in numberleſs inſtances prevails, between the act of oxigenation in bodies, and that of combuſtion, inaſmuch as neither the phænomena attending them, nor the reſults ariſing therefrom, are the ſame. The French chemiſts, however, ſeem to have been miſled, in their confining the term combuſtion to the act of oxigenation, by conſidering, that all bodies during their combuſtion combine with oxigen, without at the ſame time recollecting, that this latter effect may be produced without any of the phænomena uſually attendant on combuſtion, and that though certainly all combuſtion preſuppoſes the combination of oxigen with a baſe, yet this combination may, and repeatedly is effected where no combuſtion can poſſibly take place.

That a diſtinction therefore prevails between the two is obvious, and the Doctor offers us a theory, which he conſiders as ſufficient to explain the different phænomena produced.—This theory it is the purpoſe of the following lines to ſhew however ingenious, aud apparently ſatisfactory it may appear to be, is not wholly adequate to the taſk that is aſſigned to it. It will be neceſſary very ſhortly to ſtate here the outline of the theory under conſideration. In all caſes ſays Dr. Thomſon *, when heat and light are extricated during combuſtion, it will be found, that the light is furniſhed by the combuſtible or burning body, and the heat by the decompoſition of the oxigen, which forms a component part of the ſupporter, and which is eſſential to the combuſtion, and that the diſtinction that prevails between the two proceſſes of combuſtion, and of oxigenation, ariſes from the difference of the phænomena, which accompany the action of ſupporters and products upon other combuſtibles. " The ſupporters convert theſe bodies into products, and combuſtion, or the emiſſion of heat and light at the ſame time take place; whereas, the products convert combuſtibles into products, without any ſuch emiſſion.

Outline of the theory. That the light of combuſtion is furniſhed by the combuſtible body and the heat by the oxigen of ſupporters;

but that products convert combuſtibles into products *by mere oxigenation without combuſtion.*

* Philoſ. Journal, New Series, II. 10 and 92.

Now

Now as the ultimate change produced on combuſtibles by both theſe ſets of bodies is the ſame, and as the ſubſtance which combines with the combuſtible is the ſame, namely oxigen, it is evident, that the oxigen of the ſupporters contains ſomething, which the oxigen of the products wants," and this ſomething the Doctor ſuppoſes to be caloric. "In the ſame manner the combuſtibles and products reſemble each other, the chief difference between them conſiſting, in the phænomena which accompany their combination with oxigen, in the one caſe fire is emitted, and in the other not." Now ſays the Doctor, "if we recollect, that no ſubſtance but a combuſtible is capable of reſtoring combuſtion to the baſe of a product, and that at the time of its doing ſo, it always loſes its own combuſtibility, and further, that the baſe of a product does not exhibit the phænomena of combuſtion even when it combines with oxigen, we cannot avoid concluding that all combuſtibles contain an ingredient, which they loſe when converted into products, and that this loſs contributes to the fire, which makes its appearance during the converſion." This ingredient the Doctor ſuppoſes to be light. It is evident, that the two leading poſitions of this theory are, 1. That during combuſtion, all combuſtibles emit light, which previouſly formed a neceſſary ingredient to their own compoſition; and ſecondly, That the heat which is evolved during the proceſs of combuſtion, proceeds from the oxigen of the ſupporter, of which it likewiſe originally formed an eſſential ingredient.—That the heat given out during combuſtion comes from the decompoſition of the oxigen of the ſupporter, there are many reaſons for concluding. We know very well that no combuſtion will take place without the preſence of oxigen, and that the greater the quantity of oxigen abſorbed in a given time, the greater is always the heat that is evolved. Now if the heat be not ſuppoſed to come from the oxigen, why ſhould the degree of heat given out, be always proportional to the quantity of oxigen that is abſorbed, and upon what other principle can we ſo ſatisfactorily explain the effects that are produced by the Argand lamp. Theſe conſiderations, combined with the argument drawn from the maintenance of the temperature of hot blooded animals by the decompoſition of air, ſeem ſufficiently to eſtabliſh the truth of the foregoing poſition. There are, however, many difficulties that preſs againſt our implicit adoption of the other

Leading poſitions of the theory, 1. that light is originally an ingredient of combuſtibles, 2. and heat of oxigen.

Many reaſons why the heat muſt come from the oxigen of the ſupporter.

Difficulties as to the other poſition, that the light invariably

other part of this theory, *viz.* that the light emitted during combustion invariably proceeds from the burning body, and that consequently it forms no part of the supporter. It of course then follows from this theory, that light is no essential part in the composition of oxigen gas. Let us however see whether this be the case. Many facts it will be found concur to prove that the contrary is the truth. If nitric acid be exposed to the light, after some time we find that it changes colour, it becomes yellow, green, and then red, and oxigen gas is disengaged, the nitric at the same time being converted into nitrous acid. Now it is evident, that as this decomposition is of a chemical nature, the light that occasions it, either combines with the oxigen to form oxigen gas, or with the acid to form nitrous acid: as we find no dissimilarity between the nitrous acid procured by this means, or that by any other, we are necessitated to conclude that the light has combined with the oxigen, and that the latter by the same means is converted into oxigen gas. Again, it is well known, if oxigenated muriatic acid be exposed to the rays of the sun in a transparent bottle, there is disengaged from it oxigen gas; in proportion as the gas is separated the acid loses its colour and odour, and returns to the state of simple muriatic acid.—Here it is evident, that the oxigen has passed from a concrete into a gaseous state from the combination of light, and we must therefore conclude that light is a component part of oxigen gas.

proceeds from the burning body.

For the light which decomposes nitric or ox. mur. acid is concluded to have combined with the disengaged oxigen.

If phosphorus be inserted in nitric acid, the latter is decomposed, and a product of combustion, namely phosphoric acid is formed, during which process neither heat nor light are given out. This process Dr. Thomson considers as an act of oxigenation, and not of combustion, because, says he, though a product of combustion is formed, a new supporter, namely nitrous gas is evolved, and the formation of a combustible, or new supporter, constitutes one of the characteristic differences between the two processes of combustion and oxigenation. Now it is said, that in all cases of oxigenation a double decomposition takes place, the oxigen of the product combines with the base of the combustible, while the light of the combustible combines with the base of the product. The question then naturally presents itself,—what during this process becomes of the light which made a component part of the phosphorus

When phosphorus is acidified in nitrous acid, what becomes of the light of the combustible?

phoſphorus previous to its converſion into a product? It cannot combine with the new ſupporter that is evolved, becauſe it is a part of this theory, that light is no conſtituent part of ſupporters, but only of combuſtibles; it ſhould therefore have been made evident to the ſenſes, which we do not ever find to be the caſe, nor is any heat evolved; this latter effect is no doubt very eaſily explained, but what becomes of the light yet remains to be ſhewn.

Sulphurous and ſulphuric acids.

Sulphuric acid, ſays Dr. Thomſon, is a ſubſtance which from many of its properties I conclude to be a combuſtible, and not a product. This concluſion, however, does not appear to be perfectly conſiſtent with the definition the Doctor has given of combuſtion, for when ſulphur is heated in the air to the temperature of 302 degrees, it gives out light and heat, and is converted into an acid, *viz.* ſulphurous acid: this according to the theory under conſideration is a complete act of combuſtion, and therefore only a product of combuſtion, and not a combuſtible body ought to be formed. Sulphurous acid, according to La Grange, combines ſlowly with oxigen, and is converted into ſulphuric acid, but as no light and heat are rendered viſible, ought it not in this caſe rather to be conſidered as an act of oxigenation; for if light and heat were evolved in this proceſs, it ſhould appear that combuſtibles are capable of giving out a part only of their light in ſome caſes, and the whole in others, which does not appear very probable, for it cannot be doubted but that in ſulphurous acid, the oxigen and the ſulphur mutually ſaturate each other, and that ſulphuric acid is only ſulphurous acid combined with an additional doſe of oxigen.

Decompoſition of water by iron or zinc.

Though the Doctor apparently reconciles the decompoſition of water by iron or zinc with his theory, it yet appears to be attended with ſome difficulties which are not eaſily explained.—"Whenever, ſays he, the whole of the oxigen is abſtracted from products, the combuſtibility of their baſe is reſtored as completely as before combuſtion, but no ſubſtance is capable of abſtracting the whole of the oxigen from ſuch products, except a combuſtible, or partial combuſtible. Water, for inſtance, is a product of combuſtion whoſe baſe is hidrogen; to reſtore the combuſtibility of the hidrogen, we have only to mix water with iron or zinc filings, when the metal is oxidated, and the hidrogen gas is evolved as combuſtible as ever." Let us here attend to the phænomena which

which ſhould take place according to the Doctor's theory; water, which is a product of combuſtion, is hidrogen without its light, in union with oxigen, without its heat; by adding iron (a combuſtible containing light) we decompoſe the water, that is to ſay, 73 parts of iron unite with 27 of oxigen. Now as to every 27 parts of oxigen in water there are about four of hidrogen, of courſe theſe four parts of hidrogen are liberated; but as it does not appear probable that combuſtibles ſhould be capable of combining with light in all proportions, it may be aſked, if the 73 parts of iron which are oxidated contain juſt light enough, and no more, to reſtore the combuſtibility of the four parts of hidrogen; for if there be too much for that purpoſe, the ſuperabundant quantity ought to become viſible, and if too little, a part only of the hidrogen ſhould recover its combuſtibility, and be converted into gas. And the ſame reaſoning may of courſe be urged with regard to the decompoſition of water by zinc: for it cannot but be acknowledged, that the fact is ſomewhat ſingular, that the product of combuſtion ſhould always contain and give out the preciſe quantity of light which is ſufficient to reſtore combuſtibility to the baſe of the product, and in no caſe either more or leſs.

Numerically conſidered as to the tranſition of preciſely adequate portions of light from combuſtible to combuſtible, &c.

Thus it appears that there are many difficulties that attend our implicit aſſent to the preſent theory, and many of the phænomena of combuſtion that do not apparently admit from it of an eaſy interpretation. Whether Dr. Thomſon can reconcile theſe apparent anomalies to it, remains to be ſeen; but if it is found equal to their ſolution, there could then it ſhould ſeem be little objection to its adoption.

Concluſion.

At all events, no one will be inclined to diſpute, that the Doctor has thrown much light on a ſubject, which before its inveſtigation by him, was conſiderably more obſcure: and that he has placed it in a new point of view, which bids fair to enable us to approximate much nearer to a true theory with regard to the phænomena of combuſtion, than any other that has hitherto prevailed.

Dr. T. has thrown great light on this obſcure ſubject.

C. P.

XV.

On certain Points of Nomenclature. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

On the uſe of *y* in Engliſh for the Greek vowel *υ*.

APPREHENDING that your mode of writing certain newly impoſed names of ſubſtances in chemiſtry, ariſes from inattention, and being miſled by the French, I take the liberty of a friend to remind you, that, in Engliſh, it is not uſual to write the vowel *i* for the *υ* of the Greeks, but *y*; hence, in our language, we do not write ox*i*gen, h*i*drogen, ox*i*geniſed, &c. but ox*y*gen, h*y*drogen, ox*y*geniſed, &c.: you write, however, properly, ox*i*de inſtead of ox*y*d, as ſome perſons ſpell the word; becauſe ox*i*de ſhews the etymology in οξυς and ειδος better than ox*i*de.

Your's ever faithfully,

A. B. C.

ANSWER.

I DO not profeſs to have directed much attention to the ſubject of nomenclature; though I am well aware of its importance to the acquiſition, as well as the communication of knowledge. It is, therefore, with conſiderable diffidence that I ſtate my apprehenſion, that neither uſage in a language, nor the motive of preciſely indicating the derivation of a term, are very cogent arguments for adopting any particular mode of ſtructure, if other motives preſent themſelves. To me it ſeemed at leaſt as forcible a reaſon for the uſe of *i* inſtead of *y*, in the words alluded to, that, together with their derivatives, they are ſo very numerous, as to make it deſirable to accommodate them to the general uſage of the modern languages; and this appeared to be promoted by following the change propoſed by the framers of the chemical nomenclature.

XVI. *Duplicate*

XVI.

Duplicate Copy of a Letter from BARON DE ZACH *to the Right Honourable Sir* JOSEPH BANKS, *Bart. P. R. S. &c. transmitted to Mr.* EDWARD TROUGHTON, *and communicated by the Rev.* J. PEARSON; *on the new Planets Ceres and Pallas, with the Elements of the Orbit of the former.*

Seeberg Obſervatory, near Gotha, May 31, 1802.

MOST HONOURED SIR,

HAVING proſecuted Dr. Olber's *Pallas* from April 4 till May 11, in the meridian, Dr. Gauſs, upon this ſet of my obſervations, calculated the elements of an eliptical orbit of this very remarkable heavenly body, which repreſent, with great accuracy, all the Seeberg obſervations.

It appears, in general, by theſe calculations, that *Pallas is a planetary heavenly body, that moves between the orbits of Mars and Jupiter, with a very great eccentricity and inclination, and whoſe orbit comes very near to the orbit of the planet Ceres, perhaps touches it, perhaps even cuts it, like two links of a chain, this way* ∞, which cannot yet be aſſerted with certainty, the obſerved arc run over by this planet being too ſmall. Notwithſtanding, it appears already that the diſtances of Pallas and Ceres, in the line of nodes of their orbits, is very nearly equal. In the deſcending node, the diſtance of Pallas from the Sun is = 2,86, and the ſame diſtance of Ceres = 2,93. In the aſcending node, theſe diſtances are of greater inequality. Another very remarkable circumſtance is, that the mean motions of *Pallas* and *Ceres* are very nearly, perhaps abſolutely the ſame; though this cannot yet be aſſerted, becauſe the error of the obſervations of both planets is ſtill too great. But as far as yet appears, theſe mean motions will not differ very much; and in this caſe, ſmall as the maſſes of Ceres and Pallas may be, they will nevertheleſs exert a very ſenſible action one upon the other, and therefore give occaſion to very curious and intereſting inveſtigations in the mechanics of the heavens. The new planet *Pallas* will alſo call forth the utmoſt exertion of our analytical powers. Hitherto the two elements of a planetary orbit, *viz.* the eccentricity and the inclination, had been conſidered as an infinite little quantity, and ſo it might be, as theſe

Orbit of Pallas very eccentric, and probably linked in that of Ceres.

Intereſting remarks on the ſingularity of the orbit of Pallas.

two elements, in all our old planets, are very ſmall, ſo that the higher powers of them could be neglected without danger, in calculating their mutual action, as they produced no ſenſible term in the approximating ſeries. But this is no longer the caſe with Pallas, in which the eccentricity of the orbit and the inclination are ſo very great.

Elements of Pallas.

Elements of the Orbit of the Planet Pallas, calculated by Dr. Gauſs.

Epocha, March 31, at noon, in Seeberg,	161° 12′ 43,″2
Aphelium - - -	300 5 4, 0
Node - - - -	172 34 35, 0
Inclination - - -	35 0 42, 0
Mean daily heliocentric and tropical motion	757″ 166
Logarithm of half the greater axis -	0,4472636
Eccentricity - - - -	0,2591096

By theſe elements the whole ſeries of my obſervations are repreſented.

Computed and obſerved places.

TABLE.

1802.	R.A. by Calculation.	Difference.	Decl. by Calculat.	Difference.
Apr. 4	183° 44′ 5,″7	— 0,″9	13° 54′ 55,″4	+ 3,″4
5	183 34 24, 6	+ 0, 9	14 13 22, 6	— 0, 3
7	183 15 39, 6	+ 1, 6	14 49 1, 3	— 0, 7
8	183 6 35, 4	— 2, 4	15 6 15, 2	+ 5, 2
15	182 10 17, 9	+ 1, 4	16 54 35, 7	+ 4, 9
18	181 50 29, 4	— 1, 2	17 34 27, 1	———
19	181 44 30, 7	+ 5, 4	17 46 51, 9	— 2, 5
24	181 19 35, 8	— 2, 0	18 42 27, 9	———
25	181 15 38, 3	+ 6, 1	18 52 18, 9	———
26	181 12 2, 0	+ 0, 2	19 1 44, 9	— 4, 5
27	181 8 46, 6	+ 1, 0	19 10 46, 3	— 0, 5
29	181 3 21, 2	+ 4, 6	19 27 36, 2	— 7, 5
30	181 1 11, 4	+ 1, 5	19 35 24, 9	— 8, 6
May 1	180 59 22, 9	+ 0, 4	19 42 50, 8	+10, 0
2	180 57 56, 7	+ 4, 8	19 49 53, 2	+ 1, 6
3	180 56 52, 4	— 3, 2	19 56 32, 9	+14, 8
5	180 55 49, 6	— 5, 0	20 8 45, 6	+ 6, 9
6	180 55 51, 4	— 2, 7	20 14 19, 4	———
7	180 56 14, 9	— 8, 9	20 19 32, 1	— 3, 2
8	180 57 0, 4	— 9, 6	20 24 23, 7	+ 3, 6
11	181 1 25, 5	—16, 2	20 36 58, 8	—14, 4

The differences applied with contrary ſign to the calculated A. R. and Declin. will give the obſerved A. R. and Decl.

Pallas

Pallas and Ceres are now too near to the ſun, and the twilight permits no meridian obſervations. But aſtronomers who are provided with equatorials of great perfection, as, for inſtance, thoſe of Greenwich, Oxford, Richmond, and of Sir George Shuckburg, will be able to follow theſe two planets a longer time. The obſervation of Pallas will chiefly be of a very great value, as the ſeries of meridian obſervations is not for above five weeks. If more obſervations are not procured, it will be with ſome difficulty we ſhall find Pallas again next year; for the elements of an orbit calculated upon ſo ſmall an arc as 7½°, may give an error of ſeveral degrees in January 1803. You will do, moſt honoured Sir, a great benefit to ſcience in general, and to aſtronomy in particular, if you engage the Engliſh aſtronomers, who have ſo very excellent and fixed equatorial ſectors, to follow Pallas out of the meridian as far as they can. For this purpoſe, I take the liberty to ſend you here an ephemeris of this planet's motion, calculated by Mr. Gauſs, which will enable aſtronomers to find it, and purſue their obſervations.

These planets are now in the twilight.

Ephemeris of the Poſition of Pallas for Midnight, in Seeberg Obſervatory.

Ephemeris of Pallas.

1802.	R. Aſcenſ.	Declin.
May 24	181° 57′	21° 1′ N.
27	182 18	21 0
30	182 41	20 57
June 2	183 6	20 52
5	183 34	20 46
8	184 5	20 38
11	184 37	20 28
14	185 12	20 17
17	185 48	20 5
20	186 27	19 52
23	187 7	19 37
26	187 49	19 22
29	188 32	19 6

I am, with the greateſt eſteem and regard,

MOST HONOURED SIR,

Your moſt humble and
obedient ſervant,

FRANCIS BARON DE ZACH.

XVII. *Method*

XVII.

Method of applying a temporary Forcer to a Pump, ſo as to produce a conſtant Stream. By Mr. RICHARD TREVITHICK *. *From the Author.*

Additional forcer to a common pump producing a conſtant ſtream.

THIS contrivance which, on ſeveral occaſions, may prove uſeful, conſiſts in fixing a barrel with a ſolid piſton alongſide of the common pump, in ſuch a manner, that the lower ſpace of the additional barrel may communicate with the ſpace between the two valves of the pump, and laſtly, by connecting the rods ſo that they may work together. This is ſhewn in fig. 4, plate XI. and the effect is, that when the piſtons are raiſed, the ſpaces beneath, A and B, become filled by the preſſure of the atmoſphere, at the ſame time that the upper column flows out at E. But again, when the piſtons deſcend, the valve C ſhuts, and, conſequently, the water driven by the piſton in B muſt aſcend through A, and continue to produce an equal diſcharge through E in the down ſtroke.

Deſcription.

XVIII.

Experiments and Obſervations on certain ſtony and metalline Subſtances, which at different Times are ſaid to have fallen on the Earth; alſo on various Kinds of native Iron. By EDWARD HOWARD, *Eſq. F. R. S. From the Philoſophical Tranſactions,* 1802.

Stony and metalline ſubſtances have undoubtedly fallen on the earth.

THE concordance of a variety of facts ſeems to render it moſt indiſputable, that certain ſtony and metalline ſubſtances have, at different periods fallen on the earth. Whence their origin, or whence they came, is yet, in my judgment, involved in complete obſcurity.

The accounts of theſe peculiar Subſtances, in the early annals, even of the Royal Society, have unfortunately been blended with relations which we now conſider as fabulous; and the more ancient hiſtories of ſtones fallen from heaven,

* This Gentleman's name was, by miſtake, printed Trevithack, in Vol. I. 161.

from

from Jupiter, or from the clouds, have evidently confounded fuch fubftances with what have been termed *Ceraunia*, *Bœtilia*, *Ombria*, *Brontia*, &c. names altogether unappropriate to fubftances fallen on our globe. Indeed fome miflead, and others are inexpreffive.

Why thefe facts have been difcredited.

The term Ceraunia, by a mifnomer, deduced from its fuppofed origin, feems, as well as Bœtilia *, to have been anciently ufed to denote many fpecies of ftones, which were polifhed and fhaped into various forms, though moftly wedge-like or triangular, fometimes as inftruments, fometimes as oracles, and fometimes as deities. The import of the names, Ombria, Brontia, &c. feems fubject to the fame uncertainty.

In very early ages, it was believed, that ftones did in reality, fall, as it was faid, from heaven, or from the gods; thefe, either from ignorance, or perhaps from fuperftitious views, were confounded with other ftones, which, by their compact aggregation, were better calculated to be fhaped into different inftruments, and to which it was convenient to attach a fpecies of myfterious veneration. In modern days, becaufe explofion and report have generally accompanied the defcent of fuch fubftances, the name of thunderbolt, or thunderftone, has ignorantly attached itfelf to them; and, becaufe a variety of fubftances accidentally prefent, near buildings and trees ftruck with lightening, have, with the fame ignorance, been collected as thunderbolts, the thunderbolt and the fallen metalline fubftance have been ranked in the fame clafs of abfurdity. Certainly, fince the phenomena of lightening and electricity have been fo well identified, the idea of a thunderbolt is ridiculous. But the exiftence of peculiar fubftances fallen on the earth, I cannot hefitate to affert: and on the concordance of remote and authenticated facts, I fhall reft the affertion.

Thunderbolt.

Mr. King, the learned author of *Remarks concerning Stones faid to have fallen from the Clouds, in thefe Days and in ancient Times*, has adduced quotations of the greateft antiquity, defcriptive of the defcent of fallen ftones; and, could it be thought neceffary to add antique teftimonies to thofe inftanced by fo profound an antiquarian, the quotations of Mons. Falconet, in his papers upon Bœtilia, inferted in the *Hiftoire des Infcriptions et Belles-Lettres*;† the quotations in Zahn's

Ancient authorities of ftones fallen on the earth. *King Falconet*, &c.

* Mercati, Metallotheca Vaticana. page 241..

† Tom. VI. P. 519. et Tom. XXIII. P. 228.

fpecula

Specula Phyſico-mathematica Hiſtoriana;* the *Fiſica Sotterranea* of Giacinto Gemma; the works of Pliny, and others might be referred to.

Dr. Chaldni on the Siberian Iron &c.

Doctor Chaldni, in his *Obſervations on the Maſs of Iron found in Siberia, and ou other Maſſes of the like Kind,* as well as in his *Obſervations on Fire-balls and hard Bodies fallen from the Atmoſphere,* has collected almoſt every modern inſtance of phenomena of this nature.

M. Southey a ſtone wt. 10lbs 1796.

Mr. Southey relates an account, juridically authenticated, of a ſtone weighing 10 lb. which was heard to fall in Portugal, Feb 19, 1796, and was taken, ſtill warm, from the ground.†

Abbé Bachelay 1768.

The firſt of theſe peculiar ſubſtances with which chemiſtry has interfered, was the ſtone preſented by the Abbé Bachelay to the Royal French Academy. It was found on the 13th of September, 1768, yet hot, by perſons who ſaw it fall. It is deſcribed as follows;

"La ſubſtance de cette pierre eſt d'un gris cendré pâle; "lorſqu'on en regarde le grain à la loupe, on apperçoit que "cette pierre eſt parſemée d'une infinité de petits points bril-"lans métalliques, d'un jaune pâle; ſa ſurface extérieure, "celle qui, ſuivant M. l'Abbé Bachelay, n'étoit point en-"gagée dans la terre, étoit couverte d'une petite couche très-"mince d'une matière noire, bourſoufflée dans des endroits, "et qui paroiſſoit avoir été fondue. Cette pierre, frappée "dans l'intérieur avec l'acier, ne donnoit aucune étincelle; "ſi on frappoit, au contraire, ſur la petite couche extèrieure, "qui paroiſſoit avoir été attaquée par le feu, on parvenoit "à en tirer quelques-unes."

The ſpecific gravity of this ſtone was as 3535 to 1000.

Analyzed.

The academicians analyzed the ſtone and found it to contain.

Sulphur - - - - -	$8\frac{1}{2}$
Iron - - - - -	36
Vitrifiable earth - - -	$55\frac{1}{2}$
	100.

* Fol. 1696, Vol. I. p. 385. where a long enumeration of ſtones fallen from the ſky is given.

† Letters written during a ſhort reſidence in Spain and Portugal. p. 239.

Of

Of their mode of analyfis, I fhall have occafion to fpeak hereafter. They were induced to conclude, that the ftone, prefented to the Academy by the Abbé Bachelay, did not own its origin to thunder: that it did not fall from heaven; that it was not formed by mineral fubftances fufed by lightning: and that it was nothing but a fpecies of pyrites, without peculiarity, except as to the hepatic fmell difengaged from it by marine acid "Que cette pierre, qui peut-être étoit "couverte d'une petite couche de terre ou de gazon, aura "été frappée par la foudre, et qu'elle aura été ainfi mife en "évidence: la chaleur aura été affez grande pour fondre la "fuperficie de la partie frappée, mais elle n'aura pas été affez "long-tems continuée pour pouvoir pénétrer dans l'intérieure "c'eft ce qui fait que la pierre n'a point été décompofée. "La quantité de matiééres métalliques qu'elle contenoit, en "oppofant moins de réfiftance qu'un autre corps au courant "de matiére électrique, aura peut-être pu contribuer même à "déterminer la direction de la foudre."

Conjecture that it might have been pyrites, ftruck in preference by lightning.

The Memoir is however concluded, by obferving it to be fufficiently fingular, that M. Morand le Fils had prefented a fragment of a ftone, from the environs of Coutances, alfo faid to have fallen from heaven, which only differed from that of the Abbé Bachelay, becaufe it did not exhale the hepatic fmell with fpirit of falt. Yet the academicians did not think any conclufion could be drawn from this refemblance, unlefs that the lightening had fallen by preference on pyritical matter.*

Another ftone from Coutances.

Mons. Barthold, Profeffeur à l'Ecole centrale du Haut-Rhin, gave I believe the next, and laft, † analytical account of what he alfo denominates *Pierre de Tonnerre*. He defcribes it thus: "La maffe de pierre connue fous le nom de Pierre de "Tonnerre d'Enfisheim, pefant environ deux quintaux, a la "forme extérieure arrondie, prefque ovale, raboteufe, d'un "afpect terne et terreux.

Barthold's examination of a ftone called thunder ftone weighing two quintals.

"Le fond de la pierre eft d'une couleur grife bleuâtre, "parfemée de criftaux de pyrites, ifolés, d'une criftalifation "confufe, en quelques endroits écailleufes, ramaffés, formant

Defcription.

* See Journal de Phyfique, Tom. II. p. 251.

† A very interefting detail of a meteor, and of ftones fallen in July 1790, was given by Profeffeur Baudin, in the *Magazin für das Neuefte aus der Phyfik*, by Profeffor Voigt.

"des

" des nœuds et des petites veines, qui le parcourent en tout " ſens: la couleur des pyrites eſt doré; le poli leur donné un " éclat d'acier, et, expoſées à l'atmoſphere, elles terniſſent " et bruniſſent. On diſtingue de plus, à l'œil nud, de la mine " de fer griſe, écailleuſe, non ſulfureuſe, attirable â l'aimant, " diſſoluble dans les acids, peu oxidé, ou s'approchant beau- " coup de l'état métallique.

Fracture.

" La caſſure eſt irrégulière, grenue, d'un grain un peu " ſerré: dans l'intérieur on voit de très petites fentes. Elle ne " fait pas feu au briquet: ſa contexture eſt ſi lâche qu'elle ſe " laiſſe entamer au couteau. En la pilant, elle ſe réduit aſſez " facilement en une poudre griſe bleuâtre d'une odeur terreuſe. " Quelquefois il ſe trouve de petits criſtaux de mine de fer, " qui reſiſtent plus aux coups du pilon."

The ſpecific gravity of the piece in Profeſſor Barthold's poſſeſſion, was 3233, diſtilled water being taken at 1000.

Compoſition.

The analyſis of Mons. Barthold, of which I ſhall alſo have occaſion to ſpeak hereafter, gave in the 100.

Sulphur - - - -	2
Iron - - - - -	20
Magneſia - - - -	14
Alumina - - - -	17
Lime - - - -	2
Silica - - - - -	42
	97

From the external characters, and from his analyſis, the Profeſſor conſiders the ſtone of Enſiſheim to be argillo-ferruginous; and is of opinion that ignorance and ſuperſtition have attributed to it a miraculous exiſtence, at variance with the firſt notions of natural philoſophy.*

Sir W. Hamilton's account of ſtones that fell during a thunder ſtorm.

The account next in ſucceſſion is already printed in the Tranſactions of the Royal Society; but cannot be omitted, as it immediately relates to one of the ſubſtances I have examined. I allude to the letter received by Sir William Hamilton, from the Earl of Briſtol, dated from Sienna, July 12th, 1794. " In the midſt of a moſt violent thunder-ſtorm, " about a dozen ſtones, of various weights and dimenſions, " fell at the feet of different perſons, men, women, and " children. The ſtones are of a quality not found in any part

* See Journal de Phyſique. Ventose, An. 8. p. 169.

of

" of the Siennese territory; they fell about eighteen hours
" after the enormous eruption of Mount Vesuvius: which
" circumstance leaves a choice of difficulties in the solution
" of this extraordinary phenomenon. Either these stones
" have been generated in this igneous mass of clouds, which
" produced such unusual thunder; or, which is equally incre-
" dible, they were thrown from Vesuvius, at a distance of at
" least 250 miles; judge then of its parabola. The philoso-
" phers here incline to the first solution. I wish much, Sir,
" to know your sentiments. My first objection was to the
" fact itself; but of this there are so many eye witnesses, it
" seems impossible to withstand their evidence." (Phil. Trans. for 1795. p. 103.) Sir William Hamilton, it seems, also received a piece of one of the largest stones, which weighed upwards of five pounds; and had seen another, which weighed about one. He likewise observed, that the outside of every stone which had been found, and had been ascertained to have fallen from the clouds near Sienna, was evidently freshly vitrified, and was black, having every sign of having passed through an extreme heat; the inside was of a light grey colour, mixed with black spots and some shining particles, which the learned there had decided to be pyrites. Weight.

(To be continued.)

SCIENTIFIC NEWS, &c.

Dimensions and Nature of the New Planets Ceres and Pallas. By Dr. HERSCHEL.

DR. HERSCHEL's observations on the two lately discovered celestial bodies were read before the Royal Society, 6th of May.

Dr. Herschel begins with stating the result of his attempts to measure the diameter of the stars discovered by Piazzi and Olbers. He employed the lucid disc micrometer, which consists of an illuminated circle viewed with one eye, while the other compares with it the magnified image formed by the telescope; and he concludes, that the apparent diameter of Ceres was 22″, and of Pallas 17″ or 15″, at the distance of nearly 1.634, and 1.187 from the earth respectively, whence the apparent diameters at the distance of the earth from the sun would be .35″ and .21″ or .16″ respectively, and that their Observation of the diameters of Ceres and Pallas.

real

Ceres about 163 English miles and Pallas 95 or 71.

real diameters are about 163 and 95 or 71 English miles. There is no probability that either of these stars can have a satellite. The colour of Ceres is more ruddy than that of Pallas. They have generally more or less of a haziness, or coma, but sometimes, when the air is clear, this nebulosity scarcely exceeds the scattered light surrounding a very small star. From a view of all these circumstances, Dr. Herschel proceeds to consider the nature of the new stars. He thinks that they differ from the general character of planets, in their diminutive dimensions, in the great inclination of their orbits, in the coma surrounding them, and in the mutual proximity of their orbits; that they differ from comets in the want of eccentricity, and of a considerable nebulosity. Dr. Herschel therefore, wishes to call them asteroids, a term which he defines as a celestial body, which moves round the sun in an orbit either little or considerably eccentric, of which the plane may be inclined to the ecliptic in any angle whatever, the motion being either direct or retrograde, and the body being surrounded or not by a considerable atmosphere or a very small coma. This definition is intended to include such other bodies of the same kind as, Dr. Herschel supposes, will, in all probability, be hereafter discovered. Some additional observations show, that the apparent comas surrounding Ceres and Pallas, scarcely exceed those which are caused by aberration round the images of minute fixed stars.

Attempt to constitute a new class of celestial bodies.

J. of the Royal Institution.

Extract of a Letter from the Rev. James Wilson, D. D. Minister of Falkirk.

Falkirk, Stirlingshire, June 18*th*, 1802.

Durability of silk buried in the earth.

A few weeks ago the sexton of this parish, upon opening a grave in the church yard, found a ribband wrapped about the bone of an arm, which upon being washed was found to be entire, and to have suffered no injury, though it had lain for more than eight years in the earth; and had been in contact with a body which had passed through a state of corruption, and was reduced to its kindred dust. By what means did the silk resist the putrefactive process? it is not a compact substance like hair, horn, or bone, which are frequently found in graves after every other substance is completely changed. As silk is deprived of the gummy matter by the act of cleaning and scouring, and as this seems to be the chief animal sub-

stance

ſtance which it contains, may not the remaining fibrous part be the better prepared to withſtand the power of putrefaction? Accurate experiments tending to illuſtrate this enquiry might be both amuſing and inſtructive.

ACCOUNT OF BOOKS OF SCIENCE.

Memoirs of the Literary and Philoſophical Society of Mancheſter, Vol. V. Part II. Octavo, 700 *Pages, with* 9 *Plates.* Cadell and Davies, London, 1802.

THIS publication of the reſpectable Society of Mancheſter is no leſs intereſting than the former volumes of which the ſcientific world well knows the value; it contains the following memoirs:—1. On tragedy, and the intereſt in tragical repreſentations: An Eſſay. By the Rev. George Walker, F. R. S. and Profeſſor of Theology in the new College, Mancheſter.—2. Experiments and obſervations to determine whether the quantity of rain and dew is equal to the quantity of water carried off by the rivers and raiſed by evaporation; with an inquiry into the origin of ſprings. By Mr. John Dalton.—3. Experiments and obſervations on the power of fluids to conduct heat; with reference to Count Rumford's ſeventh eſſay on the ſame ſubject. By Mr. John Dalton.—4. Experiments on the velocity of air iſſuing out of a veſſel in different circumſtances; with the deſcription of an inſtrument to meaſure the force of the blaſt in bellows, &c. By Mr. Banks, Lecturer in Natural Philoſophy. Communicated by Mr. Dalton.—5. Eſſay on the beautiful in the human form; and enquiry whether the Grecian ſtatues preſent the moſt perfect beauty of form that we at preſent have any acquaintance with. Communicated to the Society from a Correſpondent, through the Rev. George Walker.—6. A defence of learning and the arts, againſt ſome charges of Rouſſeau: In two eſſays. By the Rev. George Walker, F. R. S.—7. Obſervations on the nervous ſyſtems of different animals; on original defects in the nervous ſyſtem of the human ſpecies, and their influence on ſenſation and voluntary motion. By John Hill, M. D.—8. Experiments and obſervations on the heat and cold produced by the mechanical condenſation and rarefaction of air. By

By Mr. John Dalton.—9. Account of ſome antiques lately found in the river Ribble. By Mr. Thomas Barritt.—10. Experimental eſſays on the conſtitution of mixed gaſes; on the force of ſteam or vapour from water and other liquids in different temperatures, both in a torricellian vacuum and in air; on evaporation; and on the expanſion of gaſes by heat. By Mr. John Dalton.—11. A review of ſome experiments, which have been ſuppoſed to diſprove the materiality of heat. By Mr. William Henry.—12. An inveſtigation of the method whereby men judge, by the ear, of the poſition of ſonorous bodies relative to their own perſons. By Mr. John Gough. Communicated by Dr. Holme. 13. On the theory of compound ſounds. By Mr. John Gough. Communicated by Dr. Holme. —14. Meteorological obſervations, made at Mancheſter. By Mr. John Dalton.—Appendix, I. Explanation of a Roman inſcription, found in Caſtle-field, Mancheſter. By Mr. Thomas Barritt. With a note on the ſame ſubject, by Dr. Holme.—II. Note to Mr. W. Henry's paper on heat.

THE Rev. Thomas Falconer, A. M. of Bath, propoſes to print by ſubſcription, the Geography of Strabo, in ſeventeen books: tranſlated from the Greek text; illuſtrated by maps, coins, inſcriptions, &c. accompanied with the notes of the older editors, and of the later; thoſe of Thomas Falconer, Eſq. of Cheſter, the Oxford editor, entire; of Siebenkees, and Tzſchucke, of Germany; and thoſe of the tranſlator.

The conditions are—1. The work will be printed in a handſome manner, with foot notes: 2. It will be contained in three volumes quarto, if poſſible: 3. The price will depend upon the Rate of paper when the work ſhall be put to preſs; but it is hoped that four guineas will be the largeſt eſtimate: 4. Two guineas to be paid at the time of ſubſcribing, for which a receipt ſhall be given, and the remainder when half the work is delivered to ſubſcribers: 5. The work will not be ſent to preſs till three hundred copies are engaged, and only five hundred will be printed. Subſcriptions received by Meſſrs. Cruttwell, and Bull, Bath; Cooke, Hanwell, and Parker, Oxford; Cadell and Davies, London; and Manners and Millar, Edinburgh.

ERRATA.

In Mr. Chenevix's paper, p. 114, for *ſulphurated* read every where *ſulphuretted.*

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST, 1802.

ARTICLE I.

On Granite. By Mr. Robert Jameson. *Communicated by the Author.*

Sheriff Bræ; Leith, July 10, 1802.

THE primitive rocks, of which granite is the oldest, were formed during that period which Werner terms the *chaotic*, when the earth was still covered to a great height with water, and before organization had commenced. Their structure shews that they have been deposited from a state of chemical solution *, and the diminishing level of the newer strata, that

Primitive rocks formed during the *chaotic* state by deposition from an aqueous solvent.

* To the idea of all fossil substances having been in a state of chemical solution in water, it has been objected, that many of them are intirely insoluble in water. To this, without adducing any of the numerous geological proofs, I answer, that we know not the original state of the different earthy and metallic substances; the artificial means we employ to procure them, may, and certainly has altered many of them from their original state.---J.

It may also be remarked, that many insoluble compounds are deposited by the chemical action of bodies which were soluble before they met. Our earths may be such compounds.---N.

 the

the water has ſunk gradually and calmly. They uſually occupy the higher parts of the globe, but when covered with newer ſtrata may form the loweſt. The rocks which Werner conſiders as belonging to this great claſs are, granite, gneiſs, mica ſlate, primitive ſlate, porphyry, and ſienite.

Granite.

As granite is in many reſpects one of the moſt important of theſe formations, I ſhall here detail ſeveral intereſting particulars reſpecting it.

On the name granite.

Pliny, and the older writers, deſcribe this rock under different names; the term granite appears to be of modern date, as Montfaucon is the firſt writer who uſes it. This will not ſurprize us, when we conſider, that it was not until the revival of letters when the remains of antiquity began to be ſtudied, that the different rocks received particular denominations. To Werner we are indebted for the moſt exact deſcription; before his time it was confounded with ſienite and grunſtone, two rocks that differ both in their oryctognoſtical and geognoſtical characters.

Granite is an aggregate, granular, primitive rock, which is compoſed of felſpar, quartz, and mica.

Aggregate component parts of granite; felſpar, quartz, and mica.

Felſpar is generally the prevailing, and mica the leaſt conſiderable ingredient. The felſpar has a conſiderable range of colour; the principal colours are white, grey, red, and ſometimes, though rarely, green: it is found in all the intermediate ſtates, from very great to very fine grain. The quartz and mica are generally grey, and the firſt has ſometimes a black colour. Several foſſils occur in granite beſides thoſe we have juſt mentioned; theſe are ſhorl and garnet: ſuch varieties have received particular names, but the geognoſt views them as accidental, and does not take particular notice of them. The topaz, which is diſtinguiſhed from all other precious ſtones (excepting the emerald and garnet) by its occurrence in primitive mountains, is found accompanied with apatite in the tin beds which lie in granite at Ehrenfredersdorf.

Structure. Porphyritic.

Its *ſtructure* is not ſubject to much variety. When cryſtals of felſpar are immerſed in a baſis of fine grained granite, it conſtitutes what is termed porphyritic granite. Of this there are fine examples near to Carlſbad in Bohemia, and in many places in the north of Scotland.

Globular.

It ſometimes occurs in globular

bular diſtinct concretions *, and theſe are again compoſed of concentric lamellar, diſtinct concretions. This ſtructure of granite is only to be diſcovered, after the ſofter granite has weathered out; then theſe concretions which are vaſtly harder, and are only ſeparated from each other by the looſer granite, make their appearance. Upon the road between Dreſden and Bautzen I obſerved many fine examples of this ſtructure of granite: Mr. Barraud, in his intereſting deſcription of the Cape of Good Hope, mentions ſeveral globular diſtinct concretions of immenſe ſize. In Scotland the iſland of Arran affords inſtances of this kind †.

Diſtinctly ſtratified.

It is frequently obſerved diſtinctly ſtratified: in other inſtances owing to the thickneſs of the ſtrata, this ſtructure is difficultly obſervable, and has given riſe to the opinion that ſuch granite is not ſtratified. The Rieſengebirge, which ſeparate Sileſia from Bohemia, are for 150 miles compoſed of granite, diſpoſed in horizontal ſtrata. Laſt ſummer I examined theſe mountains along with a conſummate mineralogiſt, Dr. Mitchell, and we convinced ourſelves of the truth of this obſervation. I have obſerved ſimilar ſtratification in Saxony and Luſatia.

It ſeldom contains extraneous beds.

It is an intereſting fact in the natural hiſtory of granite, that it ſeldom contains extraneous beds, and Werner remarks, that the frequency of ſuch beds increaſes with the newneſs of the formation: thus gneiſs contains fewer beds than mica ſlate, and mica ſlate fewer than primitive ſlate.

No limeſtone.

Limeſtone, which accompanies all the newer primitive formations, is intirely wanting in granite.

Metals which occur in Granite.

Metals which occur in granite;

This rock is not ſo rich in metals and their ores, as the primitive rocks of newer formation. It contains, however, a conſiderable variety, and ſome of theſe have been as yet only diſcovered in granite. Iron, which is remarkable on account of its occurrence in every period of the earth's formation, is found interſperſed in the oldeſt granite. Red iron ore occurs

Iron.

* Theſe globular diſtinct concretions uſed formerly to be conſidered as bowlded ſtones, and afforded an invaluable opportunity for the framing of extravagant hypotheſis.

† Mineralogy of the Scottiſh Iſles, vol. i. p. 42.

Molybdena hitherto found only in granite.

in veins in granite, alfo the brown iron ore, but this is far feldomer than the red. Molybdena has as yet only occurred in granite, either interfperfed, or in veins of the oldeft formation, as at Schlackenwalde, Geya, and Altenberg.

Bifmuth, cobalt, blende, galena, and particularly tin.

Bifmuth, cobalt, blende, galena, and feveral ores of copper have been found in granite. Of all metals, however, tin is the one moft frequently found in granite, and in the great mining field of Cornwall, it is ohferved, that copper occurs frequently in primitive flate, but the tin in granite.

The preceding obfervations refer principally to the old granite, which, as far as our experience goes, is the oldeft of all the rock formations. Werner has difcovered other granite formations, which are of a newer date.

More modern granite, alternating with gneifs.

1ft. Upon the Schneekoppe, the moft elevated part of the Riefengebirge, which is about 5000 feet above the level of the fea; granite alternates with gneifs, and hence Werner confiders it a diftinct formation *.

containing flate.

2d. At Greifenftein in Upper Saxony, Werner obferved granite which contained pieces of flate lying over ftrata of primitive flate, hence he juftly reckons it to be a diftinct formation, which is newer than either of the preceding.

Veins of granite traverfing ftrata of mica flate, and primitive flate.

3d. At Auerfberg, near Eibenftock in Saxony, and at Faftenberg near Johangeorgenftald, Werner difcovered veins of granite traverfing ftrata of mica flate and primitive flate, and this he is at prefent inclined to confider a new formation. In Scotland granite veins are very common, and feveral circumftances lead me to believe, that thefe and the Auerfberg are the fame formation. They are probably both connected with the Greifenftein formation. Werner mentions a few particulars, which he confiders as characteriftical for the newer granite formations.

Characters of the newer granite; ufually low, red, fine grained, &c.

1. Granite, which occurs in low fituations, may be fufpected to belong to the Greifenftein formation.

2. The newer granite has generally a deep red colour, is more frequently fine than coarfe grained, contains garnets, and is not porphyritic.

* I was fo fortunate as to have the opportunity of examining this formation in company with Dr. Mitchell. We obferved the granite alternations three times with the gneifs.

The

The late Dr. Hutton of Edinburgh has given us a very different geological view of granite from that contained in the preceding pages. In my outline of the mineralogy of the Scottiſh iſles, I endeavoured to ſhew the fallacy of Dr Hutton's opinions. Profeſſor Playfair, however, in his illuſtrations of the Huttonian theory lately publiſhed, has ſtated a number of facts, which he conſiders as fully confirming Dr. Hutton's ideas reſpecting granite. I therefore take this opportunity to ſtate, more fully, the reaſons that incline me to differ intirely from theſe gentlemen. Dr. Hutton conſiders granite differently.

Profeſſor Playfair is of opinion, that the diſtinction of granite into different formations is hypothetical. To this I can by no means aſſent; on the contrary, I am fully convinced, that the granite formations are well aſcertained, and this will be evident from the following obſervations: it is a poſition, the truth of which is acknowledged by every geognoſt, that of two ſeries of rocks, the neweſt, is that which covers the other. The Greifenſtein granite formation lies over the primitive ſlate, and is conſequently newer than the old granite, gneiſs, mica ſlate, and primitive ſlate. To rende rthis intelligible to thoſe unacquainted with the Wernerian geognoſie, I have repreſented this formation in the ſketch, Fig. 1. Plate XIII. Profeſſor Playfair's opinion that the preceding diſtinctions are hypothetical. The order of date in granite formation, illuſtrated from their ſituations.

From this ſketch it is plain, that after the depoſition of the old granite, gneiſs, mica ſlate, and primitive ſlate, the water had again riſen, and depoſited over the ends of theſe ſtrata, this newer granite formation.

The *ſienite formation*, which has been often confounded with the old granite, lies over the four older primitive formations, and has, interpoſed, a layer of breccia, compoſed of fragments of theſe rocks. This demonſtrates, as we have already remarked, that the water muſt have riſen and depoſited firſt the fragments of the older ſtrata, (which it had tore off in its riſing) and upon this the ſienite. The ſketch illuſtrates theſe appearances, Fig. 2, Plate XIII. Sienite formation.

Theſe facts and explanations, which are drawn from the Wernerian geognoſie, I ſhall now contraſt with thoſe of Dr. Hutton, and Profeſſor Playfair.

Profeſſor Playfair remarks, p. 309. " Veins of granite are alſo frequent in Cornwall, where they are known by the name of *lodes*, the ſame name which is applied in that country to metallic Remarks of Profeſſor Playfair on the vein of granite in Cornwall.

metallic veins. The granite veins frequently interſect the metallic, and are remarkable for producing ſhifts in them, and for throwing them out of their natural direction. The mineral veins, particularly thoſe that yield copper and tin, run nearly from eaſt to weſt, having the ſame direction with the beds of the rock itſelf, which is very hard ſchiſtus. The granite lodes, as alſo thoſe of porphyry, called *elvan*, in Cornwall, are at right angles nearly to the former; and it is remarked that they generally heave the mineral veins, but that the mineral veins ſeldom or never heave the croſs veins. In this country, therefore, veins of granite and porphyry are poſterior in formation to the metallic veins. Theſe veins of granite may perhaps be connected with the great granitic maſs that runs longitudinally through Cornwall, from Dartmoor to the Land's End. This much is certain, that their directions in general are ſuch, that if produced, they would interſect that maſs, nearly at right angles." Further, at p. 317. he remarks, "The laſt inſtance I have to mention from my own obſervation, is at St. Michael's Mount in Cornwall. That mount is intirely of granite, thruſt up from under a very hard micaceous ſchiſtus, which ſurrounds it on all ſides; at the baſe of it a great number of veins run off from the granite, and ſpread themſelves like ſo many roots fixed into the ſchiſtus: they are ſeen at low water. In the ſmaller veins, the granite is of very minute, though diſtinct parts; in the larger, it is more highly cryſtallized, and is undiſtinguiſhable from the maſs of the hill." I agree with Profeſſor Playfair, in believing that the Corniſh veins may ſtand in connection with the granite of the country, but ſhould this be proved, the Corniſh granite muſt then be referred to the Greifenſtein or Sienite formation. The appearances obſerved at St. Michael's Mount demonſtrate, that it belongs to one of the formations I have juſt mentioned; and I have no doubt that if Profeſſor Playfair had examined the ſituation of the primitive ſlate, with regard to the granite, he would have found it covered by the granite.

Iſſuing like roots from St. Michael's Mount into the ſchiſtus,

referable to the Sienite formation.

Other granite veins in Galloway,

Again at p. 316, Mr. Playfair informs us, "that another ſeries of granite veins is found in Galloway, which were firſt diſcovered by Dr. Hutton, and his friend Mr. Clerk, and afterwards more fully explored by Sir James Hall and Mr. Douglas, the preſent Earl of Selkirk. The two laſt traced the

the line of separation between a mass of granite and the schistus incumbent upon it, all around a tract of country, about eleven miles by seven, extending from the banks of Loch Ken westward; and in all this tract they found, "that wherever the junction of the granite with the schistus was visible, veins of the former, from fifty yards to the tenth of an inch in width, were to be seen running into the latter, and pervading it in all directions, so as to put it beyond all doubt, that the granite of these veins, and consequently of the great body itself, which was observed to form with the veins one uninterrupted mass, must have flowed in a soft and liquid state into its present position *. I have only further to add, that some of these veins are remarkable for containing granite, not sensibly different, in any respect, from the mass from which they proceed." The Criffle in Galloway, which is one of the most considerable portions of what Dr. Hutton considers as the granite of that country, I found to be sienite, consequently it has no relation to the old granite formation. Professor Playfair, who examined the appearances at Loch Ken, believes with Dr. Hutton, that sienite and granite, in a geognostic point of view, are to be considered as the same †. From this I draw the conclusion, that at Lock Ken we have a portion of the same sienite as that which forms the Criffle.

which Mr. Playfair concludes to have flowed in a soft state to their present situation.

The author observed the Criffle to be sienite,

and infers that the other granites are a portion of the same.

It appears then evident, that wherever granite, in the form of veins is to be observed issuing from granite into the contiguous strata of gneiss, mica slate, &c. it must belong to a newer formation, and probably to that of Greifenstein. Many of the instances where such appearances have been observed, certainly belong to the sienite formation.

The same inference generally stated,

It is therefore demonstrated, that Granite is the oldest Rock with which we are acquainted.

and that granite is the oldest of rocks.

Before I conclude these remarks, I shall notice two objections which have been urged against the possibility of granite veins having been filled from above. Mr. Playfair observes at page 313, "that a strong objection to the supposed origin of granitic veins from infiltration, and indeed to their formation in any way but by igneous fusion, arises from the number of

To Professor Playfair's remark that fragments of schistus could be insulated in granite veins only by igneous fusion;

* Transactions of the Royal Society of Edinburgh, vol. iii. p. 8.
† Illustration of the Huttonian Theory, p. 312.

fragments

fragments of ſchiſtus, often contained, and completely inſulated in theſe veins. How theſe fragments were introduced into the fiſſures of the ſchiſtus, and ſuſtained till they were ſurrounded by the matter depoſited by water, is very hard to be conceived; but if they were carried in by the melted granite, nothing is more eaſily underſtood." To this objection, it is only neceſſary to anſwer, that the apparent inſulation of maſſes of ſtone in veins, is occaſioned by our only ſeeing part of the maſs; for when the whole is ſeen, we invariably find, that it reſts upon another fragment, or upon the ſides of the veins. At p. 314, when deſcribing the junction of the granite with the other primitive ſtrata in the iſland of Arran, Profeſſor Playfair remarks, "Along this line, particularly on the ſouth, wherever the rock is laid bare, and cut into by the torrents, innumerable veins of granite are to be ſeen entering into the ſchiſtus, growing narrower as they advance into it; and being directed in very many caſes, from below upwards, they are preciſely of the kind which the infiltration of water could not produce, even were that fluid capable of diſſolving the ſubſtances which the vein conſiſts of. From the ſouth face of this mountain, and from the bed of a torrent which interſects it very deeply, Dr. Hutton brought a block of ſchiſtus, of ſeveral hundred weight, curiouſly penetrated by granite veins, including in them many inſulated fragments of the ſchiſtus."

it is anſwered, that they are not inſulated.

And with regard to the granitic veins or numerous maſſes in ſchiſtus in the iſle of Arran;

it is remarked that they are often inſulated, and have other ſigns of being nearly coeval with the gneiſs.

In the iſland of Arran I have had frequent opportunities of examining theſe kind of veins; they are generally found in gneiſs, are very much incorporated with it, and often both extremes of the vein are to be obſerved in the ſame bed. From this it is evident that theſe cannot be conſidered as granite veins, ſince they are nearly of coeval formation with the gneiſs, and have no communication with any formation, older or newer than that in which they occur *. Werner remarks, that veins which are nearly coeval with the vein rock, are

* Although we find in gneiſs veins filled, and layers compoſed of a rock which, oryctognoſtically conſidered, has every character of granite, yet the geognoſt juſtly views them as varieties of gneiſs. It often happens in a mountain of gneiſs, that ſtrata occur which are not to be diſtinguiſhed from mica ſlate; theſe, however, are merely accidental, and the whole is therefore to be referred to gneiſs. This admirable mode of inveſtigation, which was firſt diſcovered by Werner, has been of the greateſt utility in geognoſia.

very

very much incorporated with it, have no falband, far less an interpofed layer of clay, (befteg) and the vein mafs differs very little from the vein rock. On the contrary, the newer the veins, the fewer are the points of their agreement, fo that at laft there is not the leaft refemblance. Thefe old veins he fuppofes to have been immediately filled out of the fame folution from which the rock had been precipitated: the contents at leaft of this fluid had not undergone any confiderable change.

R. JAMESON.

II.

Obfervations on the Converfion of Iron into Steel. In a Letter from JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c. &c.*

To Mr. NICHOLSON.

SIR,

Northumberland, America, May 22, 1802.

YOUR giving the following article a place in your valuable Journal, will oblige Your's, &c.

J. PRIESTLEY.

ACCORDING to the antiphlogiftic theory iron becomes fteel by imbibing *carbon;* to this is faid to be owing the addition to its weight in the procefs of cementation; and the flakes of black matter which remain undiffolved after a folution of fteel in diluted acid of vitriol are faid to be *plumbago,* or carbon united to iron. Antiphlogiftic theory of fteel-making.

I cannot, however, help concluding from fome late expements, that iron is converted into fteel by imbibing only phlogifton from the charcoal, and that the addition to its weight is not from *carbon,* but from *finery cinder.* Objection from late experiments and phlogiftic theory.

Having made a quantity of iron filings perfectly pure, by firft expelling from them all the air that they could be made to yield by heat, then wafhing out of them any carbonaceous matter they might contain, and expofing them again to heat, I took 120 grains of them, and heating them with a burning lens in inflammable air, found that they imbibed 18 ounce meafures 120 gr. of iron filings cleared of air by heat and wafhed were heated in inflammable air.

which they imbibed and became bright.

measures of it. In consequence of this, from being of a dark colour, they became exceedingly bright; and I concluded that they were now become *steel*, though I was not able to ascertain it by a direct experiment. But after dissolving those bright filings in diluted vitriolic acid, a quantity of black matter, as after the solution of steel, remained unaffected by it.

These left in vitriolic acid a black matter as steel does.

This being heated in inflammable air imbibed a considerable quantity of it, and then, by means of diluted vitriolic acid, gave inflammable air very copiously. This black matter had evidently the properties of finery cinder.

Its habitudes were those of finery cinder.

I then dissolved 200 grains of *broken watch springs*, which are undoubtedly pure steel, and collected from the solution three grains of black matter. Heating this in inflammable air, a great proportion of it was imbibed; and then, by means of the diluted acid, it gave out inflammable air as copiously as iron or steel filings would have done. This black matter, therefore, from the solution of steel was finery cinder, and not carbon, or plumbago. And as iron acquires weight by becoming finery cinder, and this addition of weight, I think I have proved to be from *water*, it can hardly be doubted, but that the addition of weight to iron, in being converted into steel, is from the same cause. Indeed, I believe it to be impossible to expose iron to a red heat in circumstances in which there is any possible access of water, or of air, which always contains water, without a partial calcination of it; that is without its becoming superficially at least finery cinder.

The black matter from pure steel was affected in the same manner.

It was therefore finery cinder, and not carbon.

This was evidently the difference between the result of the solution of the *watch springs*, and that of an equal weight, viz. 200 grains of *broken polished needles*, which had not undergone any calcination. For the black matter that remained from the solution of them would not have weighed a quarter of a grain. Giving colour to steel, which is done to watch springs, is always a partial calcination of the metal; and this appears from the preceding experiments to be the conversion of a part of it into finery cinder, which is the reverse of plumbago; being, according to the new theory, an *oxide* in the highest degree; whereas if plumbago contain any oxigen, it is in the lowest degree.

Bright needles afforded much less black matter than watch springs, of which the surface was blued or oxided.

III. As

III.

An Account of the Art of making Glue. In a Letter from Mr. JOHN CLENNEL.

DEAR SIR, *Newcastle, June* 21, 1802.

THE following attempt to develope the "art and mystery" of glue-making is at your service, if you think it worthy a place in your very valuable miscellany. The improvement of any manufacture depends upon its easy access to men of science, and a prudential theorist can never be better employed than in attempting to reduce to regularity or to system the manufactures that may fall under his attention. In conformity to the first principle, I made some notes whilst visiting a glue manufactory a few years ago in Southwark, and those, interwoven with the remarks on that subject of some chemists of the first respectability, I take the liberty of sending you: at the same time I must beg of you, or your correspondents, that where it may be corrected in any manner, it may be done, and I shall feel myself obliged by the attention.

Great advantages of the visitation and report of manufactories.

Glue is an inspissated jelly, made of the parings of hides or horns of any kind, the pelts obtained from furriers, and the hoofs and ears of horses, oxen, calves, sheep, &c. quantities of all which are imported in addition to the home supply, by many of the great manufacturers of this article: these are first digested in lime water, to cleanse them as far as it can from the grease or dirt they may have contracted; they are then steeped in clean water, taking care to stir them well from time to time; afterwards they are laid in a heap, and the superabundant water pressed out; then they are boiled in a large brass caldron with clean water, skimming off the dirt as it rises, and further cleansed by putting in, after the whole is dissolved, a little melted alum or lime finely powdered, which, by their detersive properties, still further purge it: the skimming is continued for some time, when the mass is strained through baskets, and suffered to settle, that the remaining impurities, if any, may subside; it is then poured gently into the kettle again, and further evaporated by boiling a second time, and skimming, until it becomes a clear but darkish brown colour: when it is thought

Glue is made from refuse of hides, &c.

Cleaned from grease by lime-water; then steeped in clean water; drained; boiled in water; clarified by alum or lime; strained; cleared by subsidence; again boiled to the requisite density; poured into moulds; cut into portions; and dried on a net.

thought to be ſtrong enough (which is known either by the length of time a certain quantity of water and materials have boiled, or by its appearance during ebullition), it is poured into frames or moulds of about ſix feet long, one broad, and two deep, where it hardens gradually as the heat decreaſes: out of theſe troughs or receivers it is cut when cold by a ſpade, into ſquare pieces or cakes, and each of theſe placed within a ſort of wooden box, open in three diviſions to the back; in this the glue, as yet ſoft, is taken to a table by women, where they divide it into three pieces * with an inſtrument not unlike a bow, having a braſs wire for its ſtring; with this they ſtand behind the box and cut by its openings, from front to back: the pieces thus cut are taken out into the open air, and dryed on a kind of coarſe net work, faſtened in moveable ſheds of about four feet ſquare, which are placed in rows in the glue-maker's field (every one of which contains four or five rows of net work); when perfectly dry and hard, it is fit for ſale.

Character of good glue.

That is thought the beſt glue which ſwells conſiderably without melting, by three or four days immerſion in cold water, and recovers its former dimenſions and properties by drying. Glue that has got froſt, or that looks thick and black, may be melted over again and refined, with a ſufficient quantity added of freſh to overcome any injury it may have ſuſtained; but it is generally put into the kettle after what is in it has been purged in the ſecond boiling. To know good from bad glue, it is neceſſary for the purchaſer to hold it between his eye and the light, and if it appears of a ſtrong dark brown colour, and free from cloudy or black ſpots, the article is good.

Judgement of the cakes.

I am, Sir,

With great reſpect,

Your's, &c.

JOHN CLENNELL.

* When the women, by miſtake, cut only two, that which is double the ſize is called a Biſhop, and thrown into the kettle again.

IV. On

IV.

On the Preparation of Indelible Ink. In a Letter from Mr. THOMAS SHELDRAKE.

To Mr. NICHOLSON.

SIR,

As your correſpondent, Mr. Cloſe, has alluded to my memoir on the nature and preparation of drying oils, it may not be unpleaſant to receive ſuch information as I am able to give reſpecting the object of his purſuit.

By experiments repeated and varied every way that my imagination can ſuggeſt, I am convinced that amber is not ſoluble in alcohol or any eſſential oil: it is ſoluble in expreſſed oils, by the proceſs deſcribed in Lewis's philoſophical commerce of the arts, but that ſolution does not dry well, and therefore will not anſwer Mr. Cloſe's purpoſe; but when diſſolved by the * well-known proceſs for making amber-varniſh, it is likely to anſwer extremely well. Amber is not ſoluble in alcohol or eſſential oil. The ſolution in expreſſed oil is not good for ink; but the ſolution for varniſh is good.

There is another ſubſtance which ſeems likely to anſwer his purpoſe very well. † Aſphaltum is a bituminous ſubſtance, perfectly black when viewed in a maſs, but a dark tranſparent brown when diſſolved: it is ſoluble in ſpirit of turpentine at a Aſphaltum promiſes to be a good material.

* The following is the moſt convenient method: Put ſmall pieces of amber into an iron ladle, ſet it on a fire till they are melted, then add ſo much of the beſt drying oil as will make it liquid, ſtir them well together, and, when cold, add ſo much ſpirit of turpentine as will make it thin enough to flow from the bruſh. The object in making varniſh is to diſcolour the amber as little as poſſible; therefore it is but little roaſted, and the lighteſt coloured drying oil is uſed; but if this ſolution of amber was uſed for making ink, the darkneſs of the colour would be an advantage, therefore the amber ſhould be thoroughly melted, and the darkeſt drying oils uſed in preference to the others. Proceſs for the oily ſolution of amber.

† Within theſe few years good aſphaltum may be procured in many ſhops in London: before that period it was unknown; the beſt of what *was ſold, and is ſtill ſold in ſome places,* was the caput mortuum of amber; other compoſitions of pitch and various reſins were likewiſe ſold for aſphaltum. If it were uſed for Mr. Cloſe's ink, care ſhould be taken to ſelect the beſt. Good aſphaltum is now eaſily procured.

low

low heat, and when diffolved runs freely from the pen: I have known fome artifts draw with this in preference to ink, becaufe its colour harmonizes better with other materials alfo ufed in drawing, and becaufe it is indelible, as it ftrikes immediately into the paper, and if it is not thick, will ftrike through it: by this means every ftroke made with it is vifible through every colour that is wafhed over it.

Afphaltum in fpirit of turpentine, rendered confiftent with folution of amber, and coloured with lamp black, would give an indelible ink.

It feems then that if a folution of afphaltum was made in fpirit of turpentine, and fo much of the folution of amber was added as would give it due confiftence, and the fineft lamp black to give it colour, a perfect ink would be formed, and poffeffing thofe properties Mr. Clofe feems to defire; for, fuppofing the other materials could be removed, fo much of the colour as depends on the afphaltum would be indelible, except by fuch means as would deftroy the paper or parchment.

The drying oil of the varnifh would increafe the difficulty of obliteration.

Even the fmall quantity of drying oil introduced into the varnifh, would be ufeful in this refpect; for it is well known, that if oil is dropped upon white paper, though the mark is fcarcely vifible at firft, in a year or two it will become a dark yellowifh brown: it feems as though the oil changes the paper fo much that its colour can never be recovered, at leaft thofe who undertake to reftore the white colour of old prints, always make an exception to fpots of oil.

If this hint fhould be thought deferving any notice, you will have the goodnefs to make what ufe you pleafe of it.

I am, Sir,

Your's, &c.

T. SHELDRAKE.

No. 50, Strand, July 6, 1802.

Copal would probably be difcharged from paper by camphorated fpirit.

P. S. I believe Mr. Clofe will find himfelf miftaken as to the infolubility of copal when ufed in his ink. So powerful is the influence of camphor upon it, that if copal be reduced to powder, and a little camphor is rubbed into it, it immediately begins to foften, and the whole foon becomes a coherent mafs; and though copal is not foluble in alcohol alone, if camphor is added, it diffolves as eafily in the compound as the fofteft refin would. It is therefore extremely probable, that, if a paper written with his ink was wafhed with camphorated fpirits, the writing would be removed with very little difficulty.

V. *Obfervations*

V.

Obſervations on the Cauſes why a large Quantity of common Salt prevents Putrefaction, and a ſmall Quantity haſtens it. By D. H.

To Mr. NICHOLSON.

SIR,

THERE are few phenomena of nature more intereſting, and at the ſame time more involved in obſcurity, than the two oppoſite actions of muriate of ſoda, which is known to have very conſiderable effect, both in accelerating and in retarding putrefaction. The *antiſeptic* property of this ſalt has been known from the earlieſt ages. It was diſcovered, however, by Pringle, Macbride, and Gardane, that putrefaction may be haſtened by ſprinkling the animal ſubſtances with water holding a ſmall quantity of muriate of ſoda in ſolution. This diſcovery excited much ſurpriſe; and the celebrated chemiſts who obſerved it ſeem to have been fully aware of the difficulties attending its explanation, as they have offered no theory to account for it. It would doubtleſs appear a preſumptuous undertaking, to attempt the ſolution of a queſtion which has baffled the ingenuity of ſo many philoſophers, did not the ſubſequent diſcoveries in chemiſtry and phyſiology enable us to ſpeculate on the ſubject with ſome degree of probability.

Remarkable oppoſition of effects of common ſalt in preventing or accelerating putrefaction; as its quantity is more or leſs.

It ſeems neceſſary for the decompoſition of an animal ſubſtance, *1ſt*, that it be in contact with atmoſpheric air; *2dly*, that it be expoſed to a moderate degree of heat; and, *3dly*, that it be impregnated with humidity. It muſt neceſſarily follow, that whatever removes theſe conditions will check the progreſs of putrefaction. Of this we have many inſtances, as in the effects of cold; in covering the ſubſtances with ſugar, reſins, &c. and in preſerving them in ſpirit of wine. I conceive with Gren, that muriate of ſoda acts only in this way, by abſtracting the moiſture, and removing the ſubſtance from the contact of oxigen; and not by a peculiar innate, and as it were hidden *(vis occulta)* antiſeptic power *.

Putrefaction requires air, warmth, and moiſture;

and is checked by cold and covering from the air.

Crude common ſalt ſuppoſed to preſerve by drying and covering the body it is applied to.

* Gren's Chemiſtry, Chap. VIII.

With

Putrefaction is hastened by destroying irritability.

With regard to the *septic* property of muriate of soda, it must be referred to another cause. The destruction of muscular irritability appears to be a chief cause of accelerating putrefaction. This was ascertained by Mr. John Hunter, who found, that when death is occasioned by an electric shock, by violent exercise, by a blow on the stomach, or by any thing that destroys the irritability of the muscular fibre, putrefaction quickly ensues. Fontana found that the same effects were produced by the poison of vipers. It has been also found by experiment, that the compounds of potash and soda destroy muscular irritability. Now, is it not a fair inference from these premises, that a small quantity of muriate of soda should possess a septic quality? Upon these grounds, it will not be difficult to reconcile the two opposite actions of muriate of soda.

Common salt inferred to produce that effect.

Much salt retards putrefaction by drying and covering, more than it accelerates by the last mentioned quality.

When a large quantity of this salt is applied to an animal substance, it acts merely by removing the indispensible conditions of putrefaction, air and moisture. The particles in contact with the substance may indeed act in destroying the irritability of the muscular fibre; but this being only a secondary cause of putrefaction, cannot operate unless in conjunction with air and moisture.

Little salt accelerates it for the contrary reason.

On the other hand, when a small quantity of this salt in solution is applied, it is insufficient either to exclude the air, or to abstract the moisture; its peculiar property, therefore, acts *in conjunction* with the other causes, and these causes united accelerate the putrefactive process much more than any of them separately.

Such is the explanation of these phenomena which occurred to me. Although it may be imperfect in many points, yet it appears to involve no hypothesis, but to be a strict deduction from facts. Should this attempt have the good fortune to meet with your approbation, its insertion in the Philosophical Journal may at least have the effect of drawing the attention of some more eminent chemist to this too much neglected subject.

I am, Sir,

Your's respectfully,

Edinburgh, July 12, 1802. D. H.

VI. *Account*

VI.

Account of the Methods by which Soda is at present prepared for the English Market; with other Observations. By Mr. FRED. ACCUM. *From the Author.*

SINCE the late new duty on salt, the manufacturers of soda avail themselves of the method of decomposing the sulphate of soda *, by means of what is called American potash. Though this kind of potash contains less pure alkali than the Russian or German potash, it is imported in a state of perfect dryness, whereas the other is always met with in a moist state; so that the absence of water appears to compensate for the deficiency of the alkali. The American potash sells now from 48s. to 54s. per cwt. it has been considerably higher, until lately it fluctuated between 54s. and 56s.

Soda obtained by decomposing the sulphate of the bleachers with American potash.

I have been employed in a soda manufactory in which the following method answered exceedingly well. Five hundred pounds of sulphate of soda were introduced into an iron boiler containing a sufficient quantity of Thames water. 560 lb. of American potash were likewise dissolved in as little water as possible in an iron boiler fixed near the former. The potash was always previously tried, and if indifferent the quantity taken was 10 lb. more.

Particular description of the manufacturing process.

The solution, as near as I recollect, was made with about thirty pails of water to the alkali here mentioned. Both solutions were then made to boil, and as soon as the ebullition took place the solution of potash was ladled into the boiler containing the sulphate of soda. The mixture was agitated during the transfusion, and the fire raised as expeditiously as possible. As soon as the fluid boiled it was ladled into a wooden gutter, which conveyed it into a cistern of wood lined with sheet lead nearly half an inch thick, which was fixed in a cool place. Sticks of wood were then placed across the cistern, from which slips of sheet lead two or three inches wide were hung into the fluid at four inches distant from each

A boiling solution of potash was added to another of sulphate of soda, then boiled,

* Sulphate of soda is sold cheap by the bleachers, who have it as the residue of decomposing common salt by sulphuric acid with manganese. In February, 1801, it was worth from 11s. to 14s. per cwt. but I have not the present price.---A.

and drawn off to cryſtallize in a leaden ciſtern.

other. When all was cool, which in the winter was generally the caſe in three days, a plug in the bottom of the ciſtern was drawn in order to let off the fluid, and the cryſtallized ſalt was taken from the ſlips of lead. The bottom exhibited a rock of ſalt, which was detached by chiſſel and mallet. On this account it is that the lead which lines the ciſtern muſt be thick, in order to guard againſt accidents. For if the metal be perforated, the ſaline ſolution creeps between it and the wood, and in a very ſhort time detaches the lining, and it is beſides extremely difficult to find out the place where the defect really is. The temperature where the ſoda is left to cryſtallize ought not to exceed 55° Fah. In this ſtage of the proceſs the whole of the ſalt is waſhed in the ſame ciſtern with cold water, to clear it of impurities; after which it is transferred again into the boiler, diſſolved in clear water, and evaporated by heat. As ſoon as a ſtrong pellicle is formed, it is ſuffered to cool ſo far that the hand may be dipped in the fluid without injury, and the heat is kept at that temperature as long as effectual pellicles continue to be formed over the whole ſurface of the boiler, and then fall to the bottom. When no more pellicles are formed, or at leaſt only by blowing with the mouth upon the ſurface, the fire is withdrawn, and the fluid is ladled out into the ciſtern to cryſtallize. The ſulphate of potaſh, &c. which had been depoſited, is then taken out of the boiler and put aſide. If the fluid be ſuffered to cool pretty low before it is ſuffered to run into the ciſtern, very little ſulphate of potaſh is found in the ſoda; but in general the rocky maſſes of ſoda met with in the market contain a conſiderable quantity. By this proceſs from 136 to 139 lb. of ſoda may be obtained from 100 lb. of ſulphate of ſoda, if the ſoda be cryſtallized in large cryſtals; if ſmall cryſtallized it yields leſs; it ſells now at 52s. or 54s. per cwt. and is retailed at 8d. per lb.

at a temperature under 55°. The ſoda is then waſhed with cold water;

then diſſolved and evaporated at a low heat.

Pellicles of ſulphate of potaſh are formed and fall down;

which ceaſing, the fluid is drawn off to cryſtallize.

100 parts of ſulphate of ſoda afford 138 of carbonate.

The two cryſtallizations are found to be neceſſary.

We might be inclined to ſuppoſe that the firſt operation was unneceſſary, and that the ſoda might be ſeparated at once from the ſulphate of potaſh at the inſtant of its formation; but practice will convince the operator otherwiſe. A conſiderable loſs is manifeſted if the proceſs be not conducted in this manner; though the diſcovery of the cauſe may perhaps be not ſo eaſily accompliſhed as the proof of the fact.

Other

Other manufacturers grind together 500 cwt. of Glauber's salt of the bleachers, and 100 cwt. of charcoal; they expose this mixture in a reverberatory furnace resembling a baking oven, till the matter when stirred with a rake becomes pasty. It is then withdrawn and transferred into large casks, each provided with a double bottom. Water is then suffered to stand one inch high over it for 24 hours; the cock is then opened, the solution runs through the perforated bottom, over which a stratum of straw had been previously placed; and is thence conducted into the boiler for evaporation and crystallization *.

Another process. The acid of sulphate of soda is decomposed and expelled by heat with charcoal, and the soda extracted by water, &c.

It is a curious fact, that iron plates are absolutely necessary to constitute the surface on which these articles are exposed to heat: fire bricks do not answer. It seems as if the iron assisted the union; though neither iron filings mixed with the articles nor pyrites are found of advantage.

The heat must be applied on an iron, and not a brick hearth.

This method of making soda is extremely uncertain. If the heat be not raised gradually, or if the mixture be not fused enough, or a little too much, it does not succeed. The worst event is, that when the mixture has been made too hot, sulphuric acid is produced, and sulphate of potash is formed.

Uncertainty of this method.

The quantity of soda which may be obtained by this process, is said to be equal to that obtained by any other method.

It is said to be profitable if well managed.

I have been lately informed, that in Germany soda is made by decomposing the sulphate of soda by means of acetite of lime; the acetic acid is obtained for that purpose from wood, and the charcoal is found to pay the costs.

Third method. Sulphate of soda decomposed by acetite of lime, of which the acid is had by distilling wood.

The method recommended by several chemists, of obtaining soda by decomposing Glauber's salt by acetite, or the oxides of lead, does not answer in this country. The mass is by far too bulky; and requires too much time, attendance, and fuel to reduce it to a narrow compass. I have been informed by men well skilled in this department, that it is nearly impracticable in the large way. The muriate of lead which is produced cannot be used as a white pigment, as the inventors pretend.

The method recommended by acetite of lead, or else oxides of that metal is productive of loss in this country. The muriate of lead thus obtained is not a pigment.

In Prussia they mix muriate of soda and quick lime together; then slake the lime, and form the whole into a thick pulp, which

Prussian method by quicklime; then slaking and long exposure to the air. Product sulphate of lime and carbonate of soda.

* This was the process at the salt works.---N.

which is extended about two inches thick over a large ſurface, and left in that ſituation for three months. Carbonate of ſoda is then formed, which is waſhed out and cryſtallized in the uſual manner. The ſoda obtained by this proceſs always has a yellowiſh caſt.

VII.

Compariſon of the French definitive Metre with an Engliſh Standard, brought from London by M. A. PICTET, *one of the Editors of the Bibliothéque Britannique**.

Short hiſtory of admeaſurements of the earth.

THE meaſurement of the earth, and the inveſtigation of its figure, were the ſubjects, at various times in the courſe of the eighteenth century, of the labours of a number of philoſophers of the firſt eminence in different countries. Some Swediſh aſtronomers are now employed in a ſecond meaſurement of the ſame degree which was meaſured ſixty years ago by the French Academicians in Lapland, under the polar circle. In France, when the idea of ſeeking in the dimenſions of the globe itſelf the unit to which all meaſures and weights might be referred, had once been conceived and adopted, it was neceſſary to make an effort proportional to the importance of an undertaking which was thus become national. In the midſt of a long and ſanguinary war, together with difficulties of every other kind, a chain of triangles has been formed between Dunkirk and Barcelona, comprehending the tenth part of the arc of the meridian which extends from the Equator to the pole, and which is equal to one fourth of the circumference of the globe; and the ten millionth part of this arc, thus determined, has been adopted for the unit of the metrical ſyſtem: it has been fixed by the conſtruction of ſtandards made of ſubſtances proper to reſiſt the attacks of time; and by a careful examination of the precise relation of the length of the metre to that of the pendulum vibrating ſeconds, on the level of the ſea, in a given latitude, the determination of this unit has been rendered independent of any accident that might deſtroy or impair

particularly that lately made in France.

and the ſtandard meaſure thence deduced.

* From No. 148 of the Bibliotheque Britanique. I avail myſelf of the free tranſlation given in the Journal of the Royal Inſtitution; but have very carefully read the proof with the original.---N.

4

impair the ſtandards repreſenting it; while in the formation of theſe ſtandards all the precautions have been employed that could be ſuggeſted by the preſent improved ſtate of natural philoſophy, and of the arts.

Operations of the ſame kind in England.

In England, on the other hand, operations have been carried on for theſe five and twenty years, which are to be the foundations of an exact map of Great Britain. Theſe labours, begun by the late General Roy, have been conducted with much ſagacity and preciſion; and the reſults are likely to procure very intereſting information reſpecting the figure of the earth. Sir George Shuckburgh, an eminent member of the Royal Society of London, has ſucceſsfully employed himſelf in private, in reſearches intended to fix the preciſe length of the ſtandards, which have ſerved as baſes for the meaſurements made in Great Britain.

Great advantage of an accurate compariſon of the French and Engliſh ſtandards.

It was therefore to be regretted, that operations ſo ſimilar, conducted in two neighbouring countries, and capable of acquiring a new intereſt by compariſon, ſhould remain unconnected, for want of an actual ſtandard of the meaſures of the one country, which might be tranſported into the other, after the definitive determination of the French meaſure. This regret we had deeply felt at various times when theſe objects were laid before our readers; and we may ſay wtth truth, that if the hope of procuring this medium of compariſon was not the only motive of the journey to England that one of us has made, it at leaſt greatly contributed to induce him to undertake it.

Means uſed by M. Pictet to obtain a true Britiſh ſtandard.

Our colleague took ſome ſteps in his paſſage through Paris, to obtain an authentic metre, in order to be ſubmitted to the examination of the Royal Society, to which he has the honour of belonging, but he did not remain long enough in Paris to be able to ſucceed in this attempt. He took advantage of his longer ſtay in England, in procuring from the hands of Mr. Troughton, an artiſt celebrated for his accuracy in the conſtruction and diviſion of geometrical and aſtronomical inſtruments, a ſtandard rigorouſly conformable to that which he had made for Sir George Shuckburgh, and with which this philoſopher had compared the principal Engliſh ſtandards.

Apparatus by *Troughton* for comparing linear meaſures.

Our colleague procured alſo from the ſame artiſt the comparative apparatus of Sir George Shuckburgh, compoſed of two excellent microſcopes, the one bearing a micrometer which divides

vides the Englifh inch into ten thoufand equal parts. Upon his return to Paris he made hafte to exhibit thefe inftruments to the Minifter of the Interior, and to the National Inftitute. This learned body nominated three of its members, in order to proceed to the regular comparifon of the definitive metre with the Englifh ftandard. The undertaking, by no means fo eafy as it at firft appeared, occupied the committee in five different meetings, of nearly four hours each; and it was performed with all the care and precaution that the nature of the fubject required. Mr. Prony, who, as the tranflator of General Roy's memoir on the firft trigonometrical operations in England, was particularly interefted in thefe refearches, acted as fecretary to the Committee, and it was at his houfe, and with the affiftance of a comparative apparatus belonging to him, that the principal experiments were made. He has been fo obliging as to furnifh us with an authentic copy of the report made to the Inftitute, which was deemed of fufficient confequence to be read at the public fitting of the laft quarter. He adds, that "This report will foon be followed by a memoir, in which he will enter into more circumftantial details of all the obfervations that he has made; and in which he will give a defcription and a figure of his comparative inftrument." We fhall bear in mind this promife, and in the mean time we fhall give our readers a copy of the report; informing them that we have beftowed on the correction of the proofs of this important paper all the attention neceffary to enable us to affirm that no typographical error has been committed in the numbers.

Commiffion of the French National Inftitute for comparing.

National Inftitute of Sciences and Arts. 6 *Nivofe, Year* 10 *(27th December,* 1801.*)*

A member read, in the name of a committee, the following report on the comparifon of the ftandard metre of the Inftitute with the Englifh foot.

Report of the Inftitute.

M. Pictet, Profeffor of Natural Philofophy at Geneva, fubmitted to the infpection of the clafs in the month of Vendémiaire, an interefting collection of objects relative to the fciences and arts, which he collected in his journey to England. Among them was a ftandard of the Englifh linear meafure, engraved on a fcale of brafs, of 49 inches in length, divided by very fine and clear lines into tenths of an inch.

Englifh ftandard of 49 inches,

It

It was made for M. Pictet by Troughton, an artist in London, who has deservedly the reputation of dividing instruments with singular accuracy; it was compared with another standard made by the same person for Sir George Shuckburgh, and it was found that the difference between the two was not greater than the difference between the divisions of each; that is, it was a quantity absolutely insensible. This standard may therefore be considered as identical with the standard described by Sir George Shuckburgh in the Philosophical Transactions for 1798.

made by Troughton.

M. Pictet also exhibited to the Institute a comparer, or an instrument for ascertaining minute differences between measures, constructed also by Mr. Troughton. It consists of two microscopes with cross wires, placed in a vertical situation, the surface of the scale being horizontal, and fixed at proper distances upon a metallic rod. One of them remains stationary at one end of the scale, the other is occasionally fixed near to the other end; and its cross wires are moveable by means of a screw, describing in its revolution $\frac{1}{100}$ of an inch, and furnished with a circular index, dividing each turn into 100 parts; so that having two lengths which differ only one tenth of an inch from each other, we may determine their difference in ten thousandths of an inch. The wires are placed obliquely with respect to the scale, so that the line of division must bisect the acute angle that they form, in order to coincide with their intersection. General Roy has described, in the 75th volume of the Philosophical Transactions, a similar instrument made by Ramsden, for measuring the expansion of metals.

Instrument of comparison bearing two microscopes.

M. Pictet offered to the class the use of the standard, with the micrometer described, for the determination of the comparative length of the metre, and the English foot: the offer was accepted with gratitude, and M M. Legendre, Méchain, and Prony, were appointed to co-operate with M. Pictet in the comparison of the standard metre of platina and the English foot.

The first meeting was on the 28th Vendémiaire (21st of October,) at the house of Mr. Lenoir.

First meeting of the commission.

At first a difficulty occurred from the different manner in which the measures were defined: the English scale was graduated by lines; the French standards were simply formed to the length of a metre: hence the length of the metre could not easily

Difficulty to compare the English scale graduated by lines, and the French, which is of the precise

length without graduation.

eaſily be taken by the microſcopes; nor could the Engliſh ſcale be meaſured by the method employed for making new ſtandard metres, which conſiſts in fixing one end againſt a firm ſupport, and bringing the other into contact with the face of a cock or ſlider, adjuſted ſo as barely to admit the original ſtandard between it and the fixed ſurface.

Method by an intermediate piece.

Mr. Lenoir attempted to overcome this difficulty by reducing to a thin edge the terminations of a piece of braſs of the length of a metre; ſo that it was compared with the ſtandard metre in the uſual manner, and its extremities, when placed on the Engliſh ſcale, conſtituted two lines parallel to thoſe which were really engraved on the ſcale, and capable of being viewed by the microſcopes.

Experimental compariſon by this method.

The ſtandard metre of platina, and another ſtandard of iron, belonging alſo to the Inſtitute, were thus compared with the Engliſh foot; each of theſe two meaſures being equal, at the temperature of melting ice, to the ten millionth part of the quadrant of the meridian. At the temperature of 15.3° of the decimal thermometer, or 59.5° of Fahrenheit, the metre of platina was equal to 39.3775 Engliſh inches; and that of iron to 39.3788, meaſured on M. Pictet's ſcale.

Reſult,

in ſome reſpects uncertain.

Theſe firſt experiments, ſhowed, however, that the method employed was liable to ſome uncertainty, ariſing from the diſficulty of placing the croſs wires preciſely at the extremity of the thin edge of the plate of braſs employed in the compariſon; a reflection or irradiation of light, which took place at that extremity, prevented its being diſtinctly obſerved if the optical axis of the microſcope was preciſely a tangent to the ſurface exactly at the termination.

Accurate method of compariſon by contacts; a ruler carrying a line was cauſed to move through the preciſe length of the metre, under the microſcopes.

In order to remove this inconvenience, another arrangement was propoſed by one of the Committee. (It was Mr. Prony that ſuggeſted this ingenious method, and M. Paul of Geneva, who happened to be preſent, that executed it. B.B.) A line was traced on a ſmall metallic ruler, perpendicular to its length; the end of the ruler was fixed againſt a ſolid obſtacle, and the croſs wires made to coincide with the line: the ſtandard metre was then interpoſed between the ſame obſtacle and the end of the piece, and the line traced on it, which had now obviouſly advanced the length of the metre, was ſubjected to the other microſcope. To the microſcopes

thus

thus fixed, the graduated ſcale was transferred; one of the diviſions was placed exactly under one of the microſcopes, and the micrometer ſcrew was turned in order to meaſure the fraction, expreſſing the diſtance of the other microſcope from another diviſion.

Preciſe reſult at 55° of Fahrenheit.

The compariſon was repeated in the ſame manner the 4th Brumaire (26th October laſt) at the houſe of one of the Committee, and after ſeveral experiments, agreeing very ſatisfactorily with each other, it was found that at the temperature 12.75° centigrade, or 55° of Fahrenheit, the ſtandard of platina was 39.3781, and that of iron 39.3795 Engliſh inches.

Reduction to the temperature of melting ice.

The two metres being conſtructed to be equal at the temperature of melting ice, theſe operations may be verified by reducing their reſults to that temperature. For this determination we are provided with the accurate experiments made by Borda, and the committee of weights and meaſures, on the dilatation of platina, braſs, and iron; from which it appears, that for every degree of the decimal thermometer, platina expands .00000856; iron .00001156; and braſs .00001783; (for Fahrenheit's ſcale theſe quantities become 476, and 642, and 990 parts in an hundred millions.) From theſe data we find that, at the freezing point, the ſtandard metre of platina was equal to 39.38280, and that of iron to 39,38265 Engliſh inches of M. Pictet's ſcale. The difference is leſs than the 500th of a line, or the 200000th of the whole metre, and is therefore wholly inconſiderable.

Standard metre at 32° is = 39.38272 Engliſh inches.

The reſult of the whole compariſon is therefore this. Suppoſing all the meaſures at the temperature of melting ice, each of the ſtandard metres is equal to the 10000000th part of the quadrant of the meridian, and to 39.38272 Engliſh inches of M. Pictet's ſcale.

At the claſs of mathematical and phyſical ſciences of the National Inſtitute, 6 Nivoſe, year 10.

Legendre, Méchain, and Prony, *Reporter*.

This report is approved, and its concluſions adopted by the claſs. Certified in conformity with the original by Delambre.

Paris, 26 Nivoſe, year 10 (16th January, 1802).

[*The reſt of this Paper is by the learned Secretary to the Royal Inſtitution, Dr. Young.*]

Reviſion by Dr. Young.

On examining the reduction of the ſtandards of platina and iron to the freezing point, it appears that they differ ſomewhat leſs

lefs than is ftated in the report, and that they coincide within an unit in the laft place of the decimals expreffing their magnitudes, or one ten thoufandth of an inch. The ftandard of platina at the freezing point becomes equal to 39.37380, and that of iron to 39.37370 Englifh inches on the fcale of brafs at 55°, and the mean of thefe to 39.37100 Englifh inches at 62°, which is the temperature that has been univerfally employed in the comparifon of Britifh ftandards, and in the late trigonometrical operations in particular. This refult agrees furprifingly with Mr. Bird's determination of the lengths of the toifes fent by Mr. Lalande to Dr. Mafkelyne, of which the mean was 76.734 inches: hence the metre, having been found to contain 36.9413 French inches, appears to be equal to 39.3702 Englifh inches: or rather to be either 39.3694 or 39.3710, accordingly as the one or the other of the two toifes happens to have been the more correct; we may therefore give the preference to that which meafured 76.736 inches.

Allowing the accuracy of the French meafurements of the arc of the meridian, the whole circumference of the globe will be 24855.43 Englifh miles, and its mean diameter 7911.73. Taking the ellipticity at $\frac{1}{230}$, the axis will be nearly $7893\frac{1}{2}$, the equatorial diameter 7928, and the diameter of a fphere of equal folid content about 7916 miles; the brafs ftandard being at the temperature of 62° of Fahrenheit.

Standard metre at 62°.

As long, therefore, as the Englifh ftandard continues to be reduced to this temperature, we muft confider the metre as equivalent to 39.3710, and not to 39.3827 Englifh inches.

Upon thefe joint authorities it may be of ufe to reprint here a table of the principal meafures and weights now ufed in France, with the very flight corrections which this laft comparifon has introduced into it. In tranflating the French terms into Englifh, we are fully at liberty to refcue them, in fome meafure, from the barbarifms in orthography which have been committed in forming them.

Meafures of length, the metre being at 32°, the foot at 62°.

French meafures of length,

	Englifh inches.
Millimetre - - - - -	.03937
Centimetre - - - - - -	.39371
Decimetre - - - - - -	3.93710
	Metre

Metre	39.37100
Decametre	393.71000
Hecatometre	3937.10000
Chiliometre	39371.00000
Myriometre	393710.00000

	M.	F.	Y.	Ft.	In.
A decametre is	0	0	10	2	9.7
A hecatometre	0	0	109	1	1
A chiliometre	0	4	213	1	10.2
A myriometre	6	1	156	0	6

8 chiliometres are nearly 5 miles.

Measures of capacity.

	Cubic inches E.
Millilitre	.06103
Centilitre	.61028
Decilitre	6.10280
Litre, a cubic decimetre	61.02800
Decalitre	610.28000
Hecatolitre	6102.80000
Chiliolitre	61028.00000
Myriolitre	610280.00000

of capacity.

A litre is nearly $2\frac{1}{8}$ wine pints. 14 decilitres are nearly 3 wine pints. A chiliolitre is 1 tun, 12.73 wine gallons.

Weights.

A gramme is the weight of a cubic centimetre of pure water at its maximum of denſity. It has been found equal to 18.827 French grains, of which 576 made 472.5 Engliſh; and 489.5058 grammes make a pound of the ſtandard of the mint at Paris.

Weights.

	E. grains.
Milligramme	.0154
Centigramme	.1544
Decigramme	1.5444
Gramme	15.4440
Decagramme	154.4402
Hecatogramme	1544.4023
Chiliogramme	15444.0234
Myriogramme	154440.2344

A deca-

A decagramme is 6 dwts. 10.44 gr. tr.; or dr. iiſs. gr. 4.44 apoth.; or 5.65 dr. avoird. A hecatogramme is 3 oz. 8.5. dr. av. A chiliogramme is 2lbs. 3 oz. 5 dr. av. A myriogramme is 22lbs. 1.15 oz. av. 100 myriogrammes are 1 ton wanting 32.8lbs.

Agrarian meaſures.

Agrarian meaſures,

Are, 1 ſquare decametre - - -	3,95 perches.
Hecatare - - -	2 acres, 1 rood, 35.4 perches.

For fire-wood.

for fire-wood.

Deciſtere, $\frac{1}{10}$ ſtere - - -	3.5317 cub. f. E.
Stere, 1 cubic metre - -	35.3171 cub. f.

Money.

MONEY. *Copper.*	E. grains.
Centime, 1 gramme - - - - -	15.4
5 centimes, or ſous - - - - -	77.2
Decime - - - - - - -	154.4
2 decimes - - - - - -	308.8

Silver $\frac{9}{10}$ or $\frac{36}{40}$ fine.

Franc, 5 grammes, - - - - -	3 dwts. 5.2 gr.
6 francs, - - - - - -	16 dwts. 2.1 gr.

The franc is nearly the ſame with the livre tournois, and worth about 10d. Bolton's penny weighs 435 gr.; his halfpenny 165; a ſhilling nearly 93 gr. and is $\frac{37}{40}$ fine.

Length of pendulum, &c.

It appears from Mr. Borda's experiments, that in latitude 45°, a pendulum of the length of a metre would perform in a vacuum 86116.5 vibrations in a day: the length of a pendulum being ſuppoſed to increaſe with the latitude, in the proportion of the ſquare of the ſine of the latitude, multiplied by .000567, while the time of its vibration remains unaltered.

Y.

VIII. *On*

VIII.

On the Figure of Sulphate of Barytes, and the Formation of Mandreporæ. In a Letter from Mr. H. SARJEANT.

To Mr. NICHOLSON.

SIR,

IF the following mineralogical notices appear to contain any thing worthy of attention, they are at your ſervice, from

Your humble ſervant,

Keſwick, July 16, 1802. H. SARJEANT.

THE barytes, or ponderous earth, occurs in a great variety of forms: one of the rareſt is deſcribed as "reſembling a num-"ber of ſmall double convex lenſes ſet edgeways in a ground." This ſingular formation, ſo different from the angular forms affected by cryſtals in general, was the cauſe of my examining it with conſiderable attention; and I am perſuaded, that whoever does the ſame, will perceive that the ſhape of theſe cryſtals is not, properly ſpeaking, lenticular, but rather a ſort of very acute edged rhomboid, inſerted by one of the corners as far as the diagonal line, ſo that the projecting part reſembles the corner of a carpenter's chiſſel, admitting that the angle formed by its edge and ſide were enlarged to 100 degrees, or ſomewhat more, and the ſide bevilled off in the ſame manner as the edge, but in the oppoſite direction.

In its moſt uſual form, the ſulphate, the barytes is a very abundant production. It is found in this and the neighbouring counties, in great quantities, with lead, with lime (common limeſtone), with iron, and with gypſum.

That ſort of limeſtone which abounds with the petrefactions called mandreporœ, appears to owe its origin to depoſitions of calcareous mud, formed in a manner ſimilar to what may be ſeen in the upper part of moſt lakes, that of Keſwick in particular, in which a ſpecies of equiſetum grows, often in ſheets of ſeveral acres, ſlightly covered with water. The ſection of the madreporœ is preciſely that of the freſh root, and they are generally inclined all in the ſame direction, as if cauſed by a ſtream of water paſſing over them, which might be the caſe when the water was drawn off from ſuch places, by the effect of earthquakes, or other great operations of nature.

The ſubſequent induration of ſuch mud into ſtone, may be conſidered as a fact ſufficiently known.

IX. *Experiments*

IX.

Experiments and Obſervations on certain ſtony and metalline Subſtances, which at different Times are ſaid to have fallen on the Earth; alſo on various Kinds of native Iron. By Edward Howard, *Eſq. F. R. S. From the Philoſophical Tranſactions.* 1802.

(Continued from page 221.)

Stone that fell in Yorkſhire and was exhibited in London. 1796, weighing 56lb. Hiſtorical account.

In 1796, a ſtone weighing 56lb. was exhibited in London, with ſeveral atteſtations of perſons who, on the 13th of December, 1795, ſaw it fall, near Wold Cottage, in Yorkſhire, at about three o'clock in the afternoon. It had penetrated through 12 inches of ſoil and 6 inches of ſolid chalk rock; and in burying itſelf, had thrown up an immenſe quantity of earth, to a great diſtance: as it fell, a number of exploſions were heard, about as loud as piſtols. In the adjacent villages, the ſounds heard were taken for guns at ſea; but, at two adjoining villages, were ſo diſtinct of ſomething ſingular paſſing through the air, towards the habitation of Mr. Topham, that five or ſix people came up, to ſee if any thing extraordinary had happened to his houſe or grounds. When the ſtone was extracted, it was warm, ſmoaked, and ſmelt very ſtrongly of ſulphur. Its courſe. as far as could be collected from different accounts, was from the ſouth-weſt. The day was mild and hazy, a ſort of weather very frequent in the Wold hills, when there are no winds or ſtorms; but there was not any thunder or lightning the whole day. No ſuch ſtone is known in the country. There was no eruption in the earth; and, from its form, it could not come from any building; and, as the day was not tempeſtuous, it did not ſeem probable that it could have been forced from any rocks, the neareſt of which are thoſe of Hamborough Head, at a diſtance of twelve miles.* The neareſt volcano, I believe to be Hecla, in Iceland.

Inveſtigations reſpecting it.

The exhibition of this ſtone, as a ſort of ſhow, did not tend to accredit the account of its deſcent, delivered in a hand-bill at the place of exhibition; much leſs could it contribute to re-

* Extracted from the printed paper delivered at the place of exhibition.

move

move the objections made to the fall of the ftones prefented to the Royal French Academy. But the Right Hon. prefident of the Royal Society, ever alive to the intereft and promotion of fcience, obferving the ftone fo exhibited to refemble a ftone fent to him as one of thofe fallen at Sienna, could not be mifled by prejudice; he obtained a piece of this extraordinary mafs, and collected many references to defcriptions of fimilar phenomena. At length, in 1799, an account of ftones fallen in the Eaft Indies was fent to the prefident, by John Lloyd Williams, Efq. which, by its unqueftionable authenticity, and by the ftriking refemblance it bears to other accounts of fallen ftones, muft remove all prejudice. Mr. Williams has fince drawn up the following more detailed narrative of facts.

Account of the Explofion of a Meteor, near Benares, in the Eaft Indies; and of the falling of fome Stones at the fame Time, about 14 *Miles from that City.* By John Lloyd Williams, Efq. F. R. S.

Explofion of a meteor near Benares, and the falling of fome ftones at the fame time.

A circumftance of fo extraordinary a nature as the fall of ftones from the heavens, could not fail to excite the wonder, and attract the attention of every inquifitive mind.

Among a fuperftitious people, any preternatural appearance is viewed with filent awe and reverence; attributing the caufes to the will of the Supreme Being, they do not prefume to judge the means by which they were produced, nor the purpofes for which they were ordered; and we are naturally led to fufpect the influence of prejudice and fuperftition, in their defcriptions of fuch phenomena; my inquiries were therefore chiefly directed to the Europeans, who were but thinly difperfed about that part of the country.

Narrative.

The information I obtained was, that on the 19th of December, 1798, about eight o'clock in the evening, a very luminous meteor was obferved in the heavens, by the inhabitants of Benares and the parts adjacent, in the form of a large ball of fire; that it was accompanied by a loud noife, refembling thunder; and that a number of ftones were faid to have fallen from it, near Krakhut, a village on the north fide of the river Goomty, about 14 miles from the city of Benares.

Large ball of fire with noife like thunder.

The meteor appeared in the weftern part of the hemifphere, and was but a fhort time vifible: it was obferved by feveral Europeans, as well as natives, in different parts of the country.

In

In the neighbourhood of Juanpoor, about 12 miles from the ſpot where the ſtones are ſaid to have fallen, it was very diſtinctly obſerved by ſeveral European gentlemen and ladies who deſcribed it as a large ball of fire, accompanied with a loud rumbling noiſe, not unlike an ill diſcharged platoon of muſquetry. It was alſo ſeen, and the noiſe heard, by various perſons at Benares. Mr. Davis obſerved the light come into the room where he was, through a glaſs window, ſo ſtrongly as to project ſhadows, from the bars between the panes, on a dark coloured carpet, very diſtinctly; and it appeared to him as luminous as the brighteſt moonlight.

Degree of light, &c.

Inveſtigation on the ſpot by Mr. Davis.

When an account of the fall of the ſtones reached Benares, Mr. Davis, the judge and magiſtrate of the diſtrict, ſent an intelligent perſon to make inquiry on the ſpot. When the perſon arrived at the village near which the ſtones were ſaid to have fallen, the natives, in anſwer to his inquiries, told him, that they had either broken to pieces, or given away to the Teſſeldar (native collector) and others, all that they had picked up; but that he might eaſily find ſome in the adjacent fields, where they would be readily diſcovered, (the crops being then not above two or three inches above the ground,) by obſerving where the earth appeared recently turned up. Following theſe directions, he found four, which he brought to Mr. Davis: moſt of theſe, the force of the fall had buried, according to a meaſure he produced, about ſix inches deep, in fields which ſeemed to have been recently watered; and it appeared, from the man's deſcription, that they muſt have lain at the diſtance of about a hundred yards from each other.

Account by the natives.

What he further learnt from the inhabitants of the village, concerning the phenomenon, was, that about eight o'clock in the evening, when retired to their habitations, they obſerved a very bright light, proceeding as from the ſky, accompanied with a loud clap of thunder, which was immediately followed by the noiſe of heavy bodies falling in the vicinity. Uncertain whether ſome of their deities might not have been concerned in this occurrence, they did not venture out to enquire into it until the next morning; when the firſt circumſtance which attracted their attention was, the appearance of the earth being turned up in different parts of their fields, as before mentioned, where, on examining, they found the ſtones.

The

The affiftant to the collector of the diftrict, Mr. Erfkine, a very intelligent young gentleman, on feeing one of the ftones, brought to him by the native fuperintendant of the collections, was alfo induced to fend a perfon to that part of the country, to make inquiry; who returned with feveral of the ftones, and brought an account fimilar to that given by the perfon fent by Mr. Davis, together with a confirmation of it from the Cauzy, (who had been directed to make the inquiry) under his hand and feal.

Inquiries by Mr. Erfkine.

Mr. Maclane, a gentleman who refided very near the village of Krakhut, gave me part of a ftone that had been brought to him the morning after the appearance of the phenomenon, by the watchman who was on duty at his houfe; this, he faid, had fallen through the top of his hut, which was clofe by, and buried itfelf feveral inches in the floor, which was of confolidated earth. The ftone muft, by his account, previous to its having been broken, have weighed upwards of two pounds.

and Mr. Maclane.

At the time the meteor appeared, the fky was perfectly ferene; not the fmalleft veftige of a cloud had been feen fince the 11th of the month, nor were any obferved for many days after.

State of the weather.

Of thefe ftones, I have feen eight, nearly perfect, befides parts of feveral others, which had been broken by the poffeffors, to diftribute among their friends. The form of the more perfect ones, appeared to be that of an irregular cube, rounded off at the edges; but the angles were to be obferved on moft of them. They were of various fizes, from about three to upwards of four inches in their largeft diameter; one of them, meafuring four inches and a quarter, weighed two pounds twelve ounces. In appearance, they were exactly fimilar: externally, they were covered with a hard black coat or incruftation, which in fome parts had the appearance of varnifh, or bitumen; and, on moft of them were fractures, which, from their being covered with a matter fimilar to that of the coat, feemed to have been made in the fall, by the ftones ftriking againft each other, and to have paffed through fome medium, probably an intenfe heat, previous to their reaching the earth. Internally, they confifted of a number of fmall fpherical bodies, of a flate colour, embedded in a whitifh gritty fubftance, interfperfed with bright fhining fpiculæ, of a metallic or pyritical nature. The fpherical bodies were much

Stones of this defcription which have been feen by the author.

Some account of the fame.

harder than the reſt of the ſtone: the white gritty part readily crumbled, on being rubbed with a hard body; and, on being broken, a quantity of it attached itſelf to the magnet, but more particularly the outſide coat or cruſt, which appeared almoſt wholly attractable by it.

The author declines any conjecture.

As two of the more perfect ſtones which I had obtained, as well as parts of ſome others, have been examined by ſeveral gentlemen well verſed in mineralogy and chemiſtry, I ſhall not attempt any further deſcription of their conſtituent parts; nor ſhall I offer any conjecture reſpecting the formation of ſuch ſingular productions, or even record thoſe which I have heard of others, but leave the world to draw their own inferences from the facts above related. I ſhall only obſerve, that it is well known there are no volcanos on the continent of India; and, as far as I can learn, no ſtones have been met with in the earth, in that part of the world, which bear the ſmalleſt reſemblance to thoſe above deſcribed.

Iron in the Bornian collection ſaid to have fallen in Bohemia.

IT remains for me to ſpeak of a ſubſtance mentioned in the *Lithophylacium Bornianum*, Part I. page 125, deſcribed thus: "Ferrum retractorium, granulis nitentibus, matrice vireſcenti "immixtis, *(Ferrum virens*, Linn.) cujus fragmenta, ab unius "ad vigenti uſque librarum pondus, cortice nigro ſcoriaceo "circumdata, ad Plann, prope Tabor, circuli Bechinenſis Bo-"hemiæ, paſſim reperiuntur."

The iron thus deſcribed, is moreover made remarkable by a note *, which obſerves, that credulous people aſſert it to have fallen from heaven during a thunder ſtorm, on the 3d of July, 1753.

The collection of Baron Born, it is well known, has a place in the cabinet of the Right Honourable Charles Greville, who, from the effect produced by comparing the hiſtories and ſtructure of the Italian and Yorkſhire ſtones with the deſcription of this iron, was induced to ſearch the collection of Born, where he diſcovered the very ſubſtance aſſerted to have fallen on the 3d of July, 1753. How far theſe four ſubſtances have reſemblance to each other, it will ſoon appear not to be my province to anticipate.

* Quæ (fragmenta) 3 Julii, anni 1753, inter tonitrua, e cœlo pluiſſe creduliores quidam aſſerunt.

The

The Preſident having done me the honour to ſubmit his ſpecimens of the Yorkſhire and Italian ſtones to my examination, I became indebted to Mr. Greville and Mr. Williams for a ſimilar diſtinction: and, being thus poſſeſſed of four ſubſtances, to all of which the ſame origin had been attributed, the neceſſity of deſcribing them mineralogically did not fail to preſent itſelf. To execute this taſk, no one could be more eager, and certainly no one better qualified, than the Count de Bournon. He has very obligingly favoured me with the following deſcriptions. Other ſtones of ſimilar origin.

Mineralogical Deſcription of the various Stones ſaid to have fallen upon the Earth. By the Count de BOURNON, *F. R. S.*

THE ſtones I am about to deſcribe, are not of any regular ſhape; and thoſe which were found in an entire ſtate, that is, thoſe which had not been broken, either by their fall or otherwiſe, were entirely covered with a black cruſt, the thickneſs of which was very inconſiderable. Count Bournon's deſcription of the various ſtones.

The ſtones which fell at Benares, are thoſe of which the mineralogical characters are the moſt ſtriking: I ſhall therefore begin the following deſcription with them; and ſhall afterwards make uſe of them, as objects of compariſon, in deſcribing the others.

STONES FROM BENARES.

Theſe ſtones, as well as the others deſcribed in this paper, whatever may be their ſize, are covered over the whole extent of their ſurface with a thin cruſt of a deep black colour: they have not the ſmalleſt gloſs; and their ſurface is ſprinkled over with ſmall aſperities, which cauſe it to feel, in ſome meaſure, like ſhagreen, or fiſh ſkin. Stones from Bernares. Thin black cruſt; rough like fiſh ſkin.

When theſe ſtones are broken, ſo as to ſhew their internal appearance, they are found to be of a greyiſh aſh colour; and of a granulated texture, very ſimilar to that of a coarſe gritſtone: they appear evidently to be compoſed of four different ſubſtances, which may be eaſily diſtinguiſhed, by making uſe of a lens. Fracture; coarſe grained, grayiſh aſh colour; compoſed of four ſubſtances; *viz.*

One of theſe ſubſtances, which is in great abundance, appears in the form of ſmall bodies, ſome of which are perfectly globular, others rather elongated or elliptical. They are of various ſizes, from that of a ſmall pin's head to that of a pea, or nearly Opake gray globules, of conchoidal fracture and ſlight luſtre, ſlightly giving fire with ſteel, and abrading glaſs;

 ſo:

ſo: ſome of them, however, but very few, are of a larger ſize. The colour of theſe ſmall globules is gray, ſometimes inclining very much to brown: and they are completely opaque. They may, with great eaſe, be broken in all directions: their fracture is conchoid, and ſhews a fine, ſmooth, compact grain, having a ſmall degree of luſtre, reſembling in ſome meaſure that of enamel. Their hardneſs is ſuch, that, being rubbed upon glaſs, they act upon it in a ſlight degree; this action is ſufficient to take off its poliſh, but not to cut it: they give faint ſparks, when ſtruck with ſteel.

Pyrites, not magnetical.

Another of theſe ſubſtances, is a martial pyrites, of an indeterminate form: its colour is a reddiſh yellow, ſlightly inclining to the colour of nickel, or to that of artificial pyrites. The texture of this ſubſtance is granulated, and not very ſtrongly connected: when powdered, it is of a black colour. This pyrites is not attractable by the magnet; and is irregularly diſtributed through the ſubſtance of the ſtone.

Metallic malleable iron.

The third of theſe ſubſtances conſiſts in ſmall particles of iron, in a perfectly metallic ſtate, ſo that they may eaſily be flattened or extended, by means of a hammer. Theſe particles give to the whole maſs of the ſtone, the property of being attractable by the magnet; they are, however, in leſs proportion than thoſe of pyrites juſt mentioned. When a piece of the ſtone was powdered, and the particles of iron ſeparated from it, as accurately as poſſible, by means of a magnet, they appeared to compoſe about $\frac{2}{100}$ of the whole weight of the ſtone.

Whitiſh gray, crumbly earth.

The three ſubſtances juſt deſcribed, are united together by means of a fourth, which is nearly of an earthy conſiſtence. For this reaſon, it is eaſy to ſeparate, with the point of a knife, or even with the nail, the little globular bodies above mentioned, or any other of the conſtituent parts of the ſtone you may wiſh to obtain. Indeed the ſtone itſelf may readily be broken, merely by the action of the fingers. The colour of this fourth ſubſtance, which ſerves as a kind of cement to unite the others, is a whitiſh gray.

The cruſt is probably black oxide of iron.

The black cruſt with which the ſurface of the ſtone is coated, although it is of no great thickneſs, emits bright ſparks, when ſtruck with ſteel: it may be broken by a ſtroke with a hammer; and ſeems to poſſeſs the ſame properties as the very attractable black oxide of iron. This cruſt is, however, like the ſubſtance of the ſtone, here and there mixed with ſmall particles of iron in

in the metallic ſtate: they may eaſily be made viſible, by paſſing a file over the cruſt, as they then become evident, on account of their metallic luſtre. This is more particularly the caſe with reſpect to the cruſt of thoſe ſtones which remain to be mentioned, they being much more rich in iron than that I have juſt deſcribed; a circumſtance I think it needleſs to repeat, in the following deſcriptions of them. The ſtone now treated of, does not, when breathed upon, emit an argillaceous ſmell: the ſame remark may be applied to all the others.

The ſpecific gravity of this ſtone is 3352.

STONE FROM YORKSHIRE.

This ſtone, the conſtituent parts of which are exactly the ſame as thoſe of the ſtones from Benares, differs from them, however, Stone from Yorkſhire. Compoſed like the former;

Firſt. In having a finer grain. but of finer grain:

Secondly. That the ſubſtance deſcribed as being in the form of ſmall globular or elliptical bodies, is not ſo conſtantly in thoſe forms, but is alſo found in particles of an irregular ſhape; a circumſtance that is not met with in the other ſtones: theſe bodies are likewiſe, in general, of a ſmaller ſize. The globules are leſs regular:

Thirdly. The proportion of martial pyrites, which has preciſely the ſame characters as that in the ſtones from Benares, is leſs; on the contrary, that of the iron in a metallic ſtate, is much greater. The quantity I was able to ſeparate by means of the magnet, appeared to me to compoſe about eight or nine parts, in one hundred, of the weight of the whole maſs. I obſerved many pieces of this iron, of a pretty conſiderable ſize; one of them, taken from a portion of the ſtone I had powdered, in order to ſeparate the iron, weighed ſeveral grains. The pyrites in leſs quantity; and the metallic iron more.

The part of the ſtone which is in an earthy ſtate, and which ſerves to connect the other parts together, has rather more conſiſtence than that of the preceding ſtones; and its appearance does not differ much from that of decompoſed felſpar or kaolin. The ſtone itſelf, therefore, although by no means hard, is rather more difficult to break with the fingers. Its earthy part is more conſiſtent.

The ſpecific gravity of this ſtone is 3508. It is denſer.

STONE FROM ITALY.

This ſtone was in a perfectly entire ſtate; conſequently, its whole ſurface was covered over with a black cruſt peculiar to Stone from Italy.

all

all ſtones of this kind. As the ſtone was of a very ſmall ſize, it became neceſſary to ſacrifice the whole of it to the inveſtigation of its nature. Its grain was coarſe, ſimilar to that of the ſtones from Benares: in it might be perceived the ſame gray globular bodies, the ſame kind of martial pyrites, and the ſame particles of iron in the metallic ſtate. The proportion of theſe laſt was much leſs than in the ſtone from Yorkſhire; but rather greater than in the ſtones from Benares. The ſame kind of gray earthy ſubſtance ſerved to connect the different parts together; and nothing more could be perceived, except a few globules, which conſiſted wholly of black oxide of iron, attractable by the magnet, and one ſingle globule of another ſubſtance, which appeared to differ from all thoſe we have already deſcribed. This laſt ſubſtance had a perfectly vitreous luſtre, and was completely tranſparent: it was of a pale yellow colour, ſlightly inclining to green; and its hardneſs was rather inferior to that of calcareous ſpar. The quantity of it, however, was too ſmall to be ſubmitted to ſuch an inveſtigation as might have determined its nature. The black cruſt which covered the ſtone, was rather thinner than that of the ſtones already deſcribed; and ſeemed to have undergone a kind of contraction, which had produced in it a number of fiſſures or furrows, thereby tracing upon the ſurface the appearance of compartments, ſimilar in ſome meaſure to what is obſerved in the ſtones called Septaria.

coarſe grained; compoſed nearly like the others.

It contained a yellow tranſparent globule, ſofter than calcareous ſpar.

The ſpecific gravity of this ſtone was 3418.

STONE FROM BOHEMIA.

Stone from Bohemia. Compoſed like the preceding;

The internal ſtructure of this ſtone is very ſimilar to that of the ſtone from Yorkſhire. Its grain is finer than that of the ſtones from Benares: in it may be obſerved the ſame gray ſubſtance, both in ſmall globules and in particles of an irregular ſhape; alſo the ſame particles of metallic iron. The ſame kind of earthy ſubſtance likewiſe ſerved to connect the other parts together.

This ſtone, however, differs materially from the others.

but its pyrites very minute; the quantity of metallic iron much greater;

Firſt. The particles of pyrites cannot be ſeen without a lens.

Secondly. It contains a much larger quantity of iron in the metallic ſtate; inſomuch, that the proportion of that metal, ſeparated from it by means of the magnet, amounted to about $\frac{25}{100}$ of the weight of the whole.

This

This ſtone has alſo (owing perhaps to its having remained a much longer time in the earth than the preceding ones, all of which were taken up nearly at the very inſtant of their fall) another difference, viz. many of the particles of iron in a metallic ſtate, have undergone an oxidizement at their ſurface; a circumſtance that has produced a great number of ſpots, of a yellowiſh brown colour, and very near to each other, over a part of its internal ſubſtance. This oxidizement, by adding to the bulk, and to the force of action, of the part we have deſcribed as ſerving by way of cement to the other conſtituent parts of the ſtone, has occaſioned a greater degree of adheſion between theſe parts, and has rendered the ſubſtance of the ſtone more compact.

and more oxided.

The great quantity of iron in a metallic ſtate which this ſtone contains, added to its greater compactneſs, makes it capable of receiving a ſlight degree of poliſh; whereas it is impoſſible to give any poliſh to the others. When poliſhed, the iron becomes very evident, in the poliſhed part; appearing in the form of ſmall ſpecks, almoſt cloſe to each other, which have the colour and luſtre peculiar to that metal: theſe ſpecks are, in general, nearly of an equal ſize.

It is harder, and admits a ſlight poliſh.

The black cruſt of this ſtone is ſimilar to that of the others.

The ſpecific gravity of the ſtone is 4281.

It is eaſy to perceive, from the foregoing deſcription, that theſe ſtones, although they have not the ſmalleſt analogy with any of the mineral ſubſtances already known, either of a volcanic or any other nature, have a very peculiar and ſtriking analogy with each other. This circumſtance renders them truly worthy to engage the attention of philoſophers; and naturally excites a deſire of knowing to what cauſes they owe their exiſtence.

Theſe ſtones are not in the ſmalleſt degree analogous with any other known minerals.

(To be continued.)

X. An

X.

An Anſwer to Mr. Gough's Eſſay on the Theory of Compound Sounds. By THOMAS YOUNG, *M. D. F. R. S.*

To Mr. NICHOLSON.

SIR,

Introductory remarks.

I HAVE already made public, through the medium of your uſeful Journal, one vindication of my remarks upon Dr. Smith's Harmonics; and I ſhall now beg you to inſert ſome further obſervations upon a ſubject nearly ſimilar. I do not think myſelf bound to reply to all the arguments that may happen to be advanced againſt any of my opinions; but when they come from perſons of literary reſpectability, and eſpecially when they convey an imputation of having detracted from the merits of others, I cannot forbear attempting to anſwer them; and in the preſent inſtance a very few words will be ſufficient for the purpoſe. Mr. Gough has publiſhed in the laſt volume of the Mancheſter Memoirs, a paper in which he profeſſes to defend Dr. Smith's opinions againſt mine: but he has unfortunately ſo far miſtaken the queſtion between us, that with reſpect to the principal circumſtances, while he imagines he is confuting me, he is completely on my ſide.

The author's inferences from coaleſcence of undulations as to the doctrines of ſound and light, ſcarcely to be called a new theory.

Proceeding upon the grounds of the well known facts relative to the grave harmonics, or the third ſounds produced by the coaleſcence of two others, as well as upon the allowed principles of the compoſition of motion, I had drawn a number of inferences, which appeared to be of ſome importance in the firſt place with reſpect to the doctrine of ſound, and which have ſince furniſhed me with ſtill more intereſting concluſions with regard to light. I was not aware that there was any probability of the juſtice of my inferences being diſputed, or that there was ſo much novelty in the mode of obtaining them, as to deſerve the name of a theory or an invention: but if it be deemed otherwiſe, I ſhall always be ready to acknowledge the invention with ſatisfaction, and to ſupport the theory with alacrity.

Dr. Smith imagined that, when undulations in

I ſhall now quote the words of Dr. Smith, which gave riſe to my animadverſions; and this ſtatement is the more neceſſary,

ſary, as I before omitted to detail the particulars of that author's miſconception, and as Mr. Gough appears wholly to have overlooked the paſſage. "Different particles of the air at the ear" ſays Dr. Smitth, "will keep moving conſtantly oppoſite ways at the ſame time. And in ſo rare a fluid as air is, where the intervals of the particles are eight or nine times greater than their diameters, there ſeems to be room enough for ſuch oppoſite motions without impediment: eſpecially as we ſee the like motions are really performed in water, which in an equal ſpace contains eight or nine hundred times as many ſuch particles as air does. For when it rains upon ſtagnating water, the circular waves propagated from different centres, appear to interſect, and paſs through, or over each other, even in oppoſite directions, without any viſible alteration in their circular figure, and therefore without any ſenſible alteration of their motions." Harmonics, 1759, p. 105.

air or in water croſs each other, different particles of the fluid were at the ſame time moved *inter ſe*, according to the reſpective undulations.

It certainly would have "coſt me an effort of ſtudy" to demonſtrate this, although I could not exactly conſider it as the "intuitive concluſion of a comprehenſive mind." Such a mind appeared to me to comprehend with equal eaſe the diſtinct and the confuſed; and I am only at a loſs to conceive how the writer of this paſſage could ever have compoſed a valuable treatiſe on optics. Mr. Gough's theory differs as widely from this ſtatement as mine does. My remark on it was this; "It is ſurpriſing that ſo great a mathematician as Dr. Smith could for a moment have entertained an idea, that the vibratious conſtituting different ſounds, ſhould be able to croſs each other in all directions, without affecting the ſame individual particles of air by their joint forces: undoubtedly they croſs, without diſturbing each other's progreſs; but this can be no otherwiſe effected than by each particle's partaking of both motions. If this aſſertion ſtood in need of any proof, it might be amply furniſhed by the phenomena of beats, and of the grave harmonics obſerved by Romieu and Tartini, which Mr. Lagrange has already conſidered in the ſame point of view." Phil. Tranſ. 1800, p. 130.

Whereas the ſame individual particles of the fluid receive and tranſmit both undulations.

I have no objection to admitting the whole of Mr. Gough's propoſitions, in the particular caſes which he has conſidered; but when he ſays that the coaleſcence of two ſounds is impoſſible on mechanical principles, he thinks proper to omit the only caſe in which I had aſſerted its exiſtence, that is, when the

Sounds may coaleſce which arrive at the ear in the ſame direction;

the ſounds arrive at the ear "in the ſame direction." p. 131. Surely Mr. Gough will not deny the poſſibility of ſuch a coincidence in direction, ſo far at leaſt that the phyſical effect may be the ſame as if the coincidence were perfect: when, for inſtance, we liſten to two or more ſounds paſſing through a long tube, or to the various ſubordinate ſounds of the ſame chord or pipe. If he be actually diſpoſed to deny the perfect coincidence in practical caſes, I ſhall only appeal to experience, which ſhows that the effect of a third ſound is moſt diſtinctly heard, whenever the theory leads us to expect it; but there will always be a much greater portion of each ſound ſo reflected from the ſurrounding objects as not to coincide in direction ſufficiently for coaleſcence, and hence the original notes will always be much more audible than the new compound. The ear indeed appears to have greater powers of analyſis than one would naturally have expected, it decompoſes a "compound" juſt as if it were a mere "mixture," not only in this caſe but in many others: how it performs this operation, I do not pretend to determine. When Mr. Gough repreſents me as maintaining that two muſical ſtrings, differing in the times of their vibration, and happening to vibrate in concert, do not occaſion two diſtinct ſounds, and that the waves of air are compelled by their mutual interference to coaleſce, thereby producing a new ſucceſſion of pulſes, conſtituting a ſingle ſound in the place of the former; his expreſſions tend to impute to me an opinion which could only be maintained by a perſon who had never heard a ſingle muſical compoſition, or ever been preſent at the converſation of a mixed company.

not by loſing their reſpective tones in one ſingle ſound; but by producing an additional weaker ſound.

All imperfect uniſons muſt produce beats.

I am not ſolicitous for the application of the term compound by coaleſcence, to the human voice; but Mr. Gough can ſcarcely form to himſelf a diſtinct conception of it, very different from mine. A mixture of imperfect uniſons would inevitably be accompanied by the production of beats; and if he aſſert that the imperfection is too ſmall to produce this effect, I will only requeſt him to aſſign any reaſonable limit to its magnitude, and by producing the note long enough, I will ſhow that a beat muſt neceſſarily enſue. Perhaps a wiſh to retain the Newtonian theory of the law of the undulations may have led him into theſe ſuperfluous refinements.

If

If Mr. Gough will take the pains of examining the phenomena of the grave harmonics, which he seems to have hitherto thought beneath his notice, he will be convinced that the coalescence of musical sounds is not only possible, but of very frequent occurrence, and that the compound sound does actually "acquire sensible properties peculiar to itself;" and if he will explain, in any other manner than I have done, the indubitable fact of the audible impression of the presence of the fourth below the key, in consequence of the coexistence of the sounds constituting the interval of a major third, even when both the notes are freed from their harmonics, and when all echo is avoided; I shall then listen to him, with the attention due to a person who endeavours to regulate his arguments by the results of accurate observation.

The phenomena of the grave harmonics shew that musical sounds coalesce in numerous instances.

I am, Sir,
with great respect,
Your obedient humble Servant,
THOMAS YOUNG.

Royal Institution, May 30, 1802.

CORRECTION.

In your fifth volume (quarto) p. 166, l. 20, *for* "a better third than that," *read*, "equal to the third."

The following CORRECTIONS *made by Dr.* YOUNG *in his Paper on the Mechanism of the Eye, which is inserted in our fifth volume (Quarto Series) were omitted to be made in their proper places.*

Page 256, line 6, Prop. III. *after e, insert* the base being unity.

Page 257, line 15, Cor. 10. *for* ntu, *read* ntt; line 16, *for* product &c. *read* square of the cosine of incidence.

Page 258, line 2, Cor. 11. *for* $1 + u^2 - 2u^4$, *read* $2muu$.

Page 258, Prop. V. Cor. See the note in p. 299.

Page 259, Prop. VIII. By a mistake of a sign, the eighth proposition is rendered erroneous; no use having been made of that proposition, it has been inserted without proper revision. It ought to stand thus, with its demonstration:

PROPOSITION VIII. PROBLEM.

To find the path of a ray of light falling obliquely on a sphere, of a refractive density varying as any power of the distance from the centre.

The

The refractive density, in the sense of these propositions, varies as the ratio of the sines, and as the velocity of light in the medium.

(Schol. 2. Prop. I.) Let the velocity at the distance x be $x^{-\frac{1}{r}}$; then, considering the refractive force as a species of attraction, we have, in Prop. 41. l. 1. Princip. $\sqrt{\overline{ABFD}} = x^{-\frac{1}{r}}$, Q $= s$, the sine of incidence, the radius being unity, Z $= sx^{-1}$, D $c =$

$$\frac{s}{2xx\sqrt{x^{-\frac{2}{r}} - s^2x^{-2}}} = \tfrac{1}{2}sx^{\frac{1}{r}-2}\cdot\overline{1-s^2x^{\frac{2}{r}-2}}\Big|^{-\frac{1}{2}},$$ and

the fluxion of the area described by the radius $= -\tfrac{1}{2}sx^{\frac{1}{r}-2}$

$\dot{x}\cdot\overline{1-s^2x^{\frac{2}{r}-2}}\Big|^{-\frac{1}{2}}$. Let the sine of the inclination to the radius at each point be called y; then $y = sx^{\frac{1}{r}-1}$, $\dot{y} = \frac{1-r}{r}sx^{\frac{1}{r}-2}$ $\dot{x}$, and the fluxion of the area $= \frac{r}{2r-2}\dot{y}\cdot\overline{1-yy}\Big|^{-\frac{1}{2}}$, of which the fluent is $\frac{r}{2r-2}$Y, y being the sine of the arc Y; and the angle corresponding is $\frac{r}{r-1}$ Y. The value of that angle being found for any two values of x or y, the difference is the intervening angle described by the radius. This angle is therefore always to the difference of the inclinations as r to $r-1$, and the deviations is to that difference as 1 to $r-1$.

Corollary. Hence, in the passage to the apsis, and the return to the surface, the deviation is always proportionate to the arc cut off by the incident ray produced: therefore such a sphere could never collect parallel rays to any focus, the lateral density being too small towards the surface.

Page 259, line 9, *for* but the two last, &c. *read* the seventh may either be deduced from the eighth, or may be demonstrated independently of it.

Page 264, last line, *after* internally, *insert* Or, if a lens of equal mean dimensions, and equal focal length, with the crystalline, be supposed to consist of two segments of the external portion of such a sphere, the refractive density at the centre of this lens must be as 18 to 17.

XI. *Experiments*

XI.

Experiments on the Velocity of Air issuing out of a Vessel in different Circumstances; with the Description of an Instrument to measure the Force of the Blast in Bellows, &c. By Mr. BANKS, *Lecturer in Natural Philosophy* *.

THE object of this inquiry may be announced in the following proposition: If an elastic fluid is generated in a given vessel, or any way contained in it, and at liberty to issue out of the said vessel through a given aperture, to determine the resistance which the vessel meets with from its action, or the power which it has of communicating motion to the vessel, as in a sky-rocket, Sadler's steam engine, &c.

Proposition. To determine the mechanic force or reaction of an elastic spouting fluid.

Before we proceed to relate the experiments, it may be proper to premise certain principles deduced from theory. If a tube be filled with any kind of fluid, as *air, water, mercury*, &c. and placed in a vacuum, every fluid will flow out with the same velocity. For though the pressure of a column of mercury of a given altitude be much greater than an equal column of water, yet the weight of the particles to be projected is greater in the same ratio. On the other hand, if air is lighter than water, the particles projected are also lighter in proportion. If a tube of 16 feet high be filled with air of any density, that air, like water, would flow into a vacuum with a velocity of 32 feet per second, no corrections being made for resistance †.

All uniform fluids flow from an orifice with the same velocity *in vacuo*, if their heights be equal, however different their densities.

And if we take the gravity of air to water as 1 to 840, then a column of one foot of water compressing air, will produce as great a velocity in that air as a column of air 840 feet high, supposing it was of uniform density.

If we take the whole pressure of the atmosphere equal to 33 feet of water, or its height (supposing it to be equally dense,

Equal velocities will be produced in one issuing fluid by the pressure of another; if the height of the former to that of the latter, be inversely as its density.

* From the Manchester Memoirs, Vol. V. p. 398.

† In the supposition of a perpendicular tube open at the top, filled with air or any elastic fluid, the author takes the density of the column at the bottom, or where the aperture is made, to arise solely from the weight of the elastic column; and the altitude to be that which would be if the whole column were reduced to the density of that at the bottom.

which

The atmosphere presses equal to 33 feet of water, or 27720 feet of air. It would therefore propel air into a vacuum with the velocity due to this fall: *viz.* $\sqrt{16} : \sqrt{27720}$ :: 32 feet : 1332 feet per second.

which in this case will make no difference) equal to 33 multiplied by 840, or 27720 feet; then as the square root of 16 is to 32, the velocity at the depth, so is the square root of 27720 to 1332 feet per second, the initial velocity of the atmosphere into a vacuum.

Experiments.

To prove whether air compressed by 33 feet of water would be impelled into the atmosphere with the above velocity, I have made, amongst many more, the following experiments:

Air is compressed in a vessel by a known height of water:

A, plate xiv. fig. 1 *, is a vessel of a known capacity, into the top of which is screwed an aperture of a known area. The tube *T d*, recurve at *d*, is soldered or screwed into the top of the said vessel. The hole *a* is stopped, and water poured into the tube at *T* till it is full, at which time a quantity of water will have passed out of the tube at *d*, and condensed the air in the vessel, more or less as the tube *T d* is longer or shorter.

and suffered to issue through an aperture of known dimensions. Its velocity is known from the quantity and the time.

At this time a person who has closed the aperture at *a* with a finger of one hand, and held a half second pendulum in the other, removes both at the same time, while at the same moment an assistant opens a cock over the tube *T*, which supplies it with water as fast as it can descend into *A*. The moment that the water appears at *a*, the time-piece is stopped, and the time of expelling the air is noted, from which, by knowing the capacity of the vessel, the velocity may be obtained.

If the tube *T d* should be continued near the bottom of the vessel *A* while it was filling with water, the length of the compressing column would be gradually diminishing, and of consequence the pressure would be constantly changing; hence the open end of the tube is as near the top of the vessel as is consistent with a free passage for the water.

EXPERIMENTS.

The vessel *A* contained 15 lb. 6 oz. of water, from which we find its capacity is 425.088 cubic inches.

The area of the aperture *a*, through which the air is expelled, .0046 inches.

* There being no figure in the Transactions, I have inserted the drawing referred to. N.

Exper.

Exper. I. The altitude of *T* above the vessel is 30 inches. Time of expelling the air, by several trials, is 33 seconds.

Experiment I. with a fall of 33 inches.

Exper. II. The altitude of *T* is six feet. The time of filling, by several trials, is 21.3 seconds.

Experiment II. with a fall of 72 inches.

In the first experiment, 425.088, the solidity of the vessel, divided by .0046, the area of the hole *a*, gives 92410.4 inches for the length of the stream of air driven out in 33 seconds; divide that length by 33, and we shall have 233.3 feet, the velocity per second, communicated by 30 inches of water.

Computation of the first experiment gives actual velocity per sec. = 233.3 feet;

The second experiment by the same process gives 361.6 feet per second. If we would compare these together, we may say, as the square root of 30, the head, is to 233.3, the velocity; so is the square root of 72, the second head, to 361.8 feet, the velocity per second.

and of the second gives 361.6 per second.

Again:—As the square root of 6 feet is to 361.6, so is the square root of 33 feet to 845.2 feet per second, the velocity produced by that head, or the initial velocity with which the atmosphere would enter a vacuum. This velocity, found by experiment, is 487 feet per second less than has been assigned by theory.

Whence the velocity of air from the atmosphere into a vacuum is computed, and turns out to be 845.2 instead of 1332 feet, as by theory.

It appears however that the results, as determined by theory and experiment, do not differ more than in the case of effluent water. For, if we would reduce the velocity of effluent water found by theory, to that which experience gives, we must multiply it by .634. Accordingly, if we multiply 1332 feet, the velocity of the atmosphere entering a vacuum as calculated above, by .634, the product is 844.5 per second, differing but $\frac{7}{10}$ of a foot from that just found by experiment.

But this result, corrected by the multiplier given by experience for effluent water, proves to be very correct.

I have also made experiments by sinking vessels in water, till their tops were even with its surface, and opening the aperture that the rising water might expel the air, by which I obtained the same velocities as above; but the method of computing is much more intricate, for which reason I shall not insert them.

Experiments with rising water.

From the above it appears, that a pressure equal to 33 feet of water, will expel air out of the bellows into the atmosphere with a velocity of 845 feet per second; that *one foot* of water in depth will produce a velocity of $147\frac{1}{4}$ feet, and *one inch* a velocity of 42 feet per second, or 20 miles an hour.

General result:

Hence we may construct a table shewing the velocity communicated to air by any head of water; for as the square root of

applied to the construction of tables for effluent air pressed by water;

of 6 feet is to the velocity produced by that head, ſo is the ſquare root of any other depth to the velocity produced by that depth.

or in bellows,

We may alſo, from the above, conſtruct an inſtrument which will ſhew the velocity with which air flows out of any kind of bellows, with as much accuracy, as the experiments have been made on which its conſtruction depends.

Deſcription of the Inſtrument, &c.

by means of an inſtrument here deſcribed. It is a portable veſſel containing water, to be formed into an open gage tube;

The metal box or tube *B*, plate xiv. fig. 2, may be about the ſize of the figure; the top muſt be made air tight by the cover *L*; into the bottom is fixed the ſmall tube *A C*, and into the piece *D* is cemented the glaſs tube *E D*; the inſtrument is then inverted, and ſome water poured through the tube *A C*, till, when in its proper poſition, it is viſible at *D*. It is now ready for uſe, and the end *A* may be fixed in a hole made in the upper board of the bellows, and the water will riſe in the glaſs tube, in ſmith's bellows, generally from 9 to 12 inches, furnace bellows, generally four feet or more. But where the compreſſion is great, quickſilver may be uſed inſtead of water, only in this caſe the inſtrument ſhould be made of iron, as quickſilver cauſes the ſcrews of braſs to break. Or, inſtead of quickſilver, the tube *E D* may be ſealed at the top *E*, and then a length of 12 or 18 inches will be enough for any blaſt. The glaſs tube needs not be more than one-eighth or one-tenth of an inch in diameter.

Whatever compreſſion there may be in the bellows, there will be the ſame in the upper part of the tube *B*, which will force the water into the glaſs tube *D E*, and make the air in its upper part of the ſame denſity, deducting from the compreſſing force the altitude of the water raiſed above *D*, which however will be of little or no importance; if the gauge is placed in a horizontal poſition, with the glaſs tube downward, there will be no difference of denſity.

or the tube may be hermetically cloſed.

The computation for the force in the caſe where a tube hermetically ſealed at the top is adopted in the inſtrument, will be effected by conſidering that the ſpace occupied by any elaſtic fluid is inverſely as its force. Thus, let the tube be 12 inches long, and ſuppoſe the water to be raiſed one inch, then it will be 11 : 12 :: the force of the atmoſphere : the force of the air in the tube :: 1 : 1$\frac{1}{11}$. Hence a ſcale may be adapted to the inſtrument,

inſtrument, to expreſs the force of condenſation over and above the common atmoſpheric preſſure; which force is ſignified in the inſtance above, by the fraction $\frac{1}{11}$, unity being the atmoſpheric preſſure. If we denote the atmoſpheric preſſure by 30 inches of mercury, or 32 feet of water, then the force $\frac{1}{11}$ in the above example, will be expreſſed by 2.727 inches of mercury, or 2.91 feet of water; and the like for any other inſtance.

If a mercurial inſtrument of the above conſtruction be preferred, it becomes neceſſary to add the height of the mercurial column to the force found as above: thus, if the condenſation of air be from 12 into 9 inches, then the addition to the force of the internal air in the tube is equal to $\frac{1}{3}$, or 10 inches of mercury, to which muſt be added the three inches raiſed in the tube, and the whole force will be 13 inches of mercury, excluſive of that of the atmoſphere.

If mercury be uſed, its denſity muſt be conſidered in the ſhort inſtrument.

This ſort of inſtrument or gauge ſerves equally well for finding the expanſive force of any kind of elaſtic fluid, as for meaſuring the velocities with which they iſſue out of the place of their confinement. It may be applied to all kinds of bellows, to condenſed ſteam, and to the air pump.

General application to all gaſes.

XII.

On the Variation of Rate in a Time-Piece, as indicated by the Changes in the Arc of Vibration. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR, *Lynn, July* 20, 1802.

FROM what I have already mentioned in a former paper *, it follows, that in clocks uſed in making aſtronomical obſervations, it is neceſſary to obſerve the arc of vibration very frequently, and when it is found different from that generally deſcribed by the pendulum, the rate of the clock muſt on this account be corrected.

Expediency of frequent obſervation of the arc of vibration in clocks.

To determine this correction, let x denote the time which a pendulum, vibrating in an arc exceedingly ſmall, will loſe when made to vibrate in a larger arc of the ſame circle, T the

Rule for deducing the variation in time from that of the arc.

* See pa. 76 of this vol.

number of ſeconds in 24 hours (86400), and D the number of degrees deſcribed on each ſide of the perpendicular *.

Then $x = T \times \frac{D^2}{52480}$ nearly. Conſequently $x = D^2 \times 1.6463$ nearly.

From the above theorem the following table was computed, which ſhews the time loſt by a pendulum in 24 hours, by increaſing its ſemi-arc of vibration in the ſame circle 1′ of a degree.

Table of vibrations, and correſpondent loſs from their increaſe.

Half the arc of vibration of a pendulum in a circle.	Loſes per day of a pendulum vibrating the leaſt arc poſſible.	Difference.	Loſes per day on increaſing its ſemi-arc of vibration 1′ of a degree.
0°	0,″0		
		0,″1	,006
0 ¼	0, 1		
		0, 3	,020
0 ½	0, 4		
		0, 5	,033
0 ¾	0, 9		
		0, 7	,046
1	1, 6		
		0, 9	,060
1 ¼	2, 5		
		1, 2	,080
1 ½	3, 7		
		1, 3	,086
1 ¾	5, 0		
		1, 6	,107
2	6, 6		
		1, 7	,113
2 ¼	8, 3		
		2, 0	,133
2 ½	10, 3		
		2, 1	,140
2 ¾	12, 4		
		2, 4	,160
3	14, 8		
		2, 6	,173
3 ¼	17, 4		
		2, 8	,187
3 ½	20, 2		
		3, 0	,200
3 ¾	23, 2		
		3, 2	,213
4	26, 3		

* Simpſon's Fluxions, Art. 464.

The

The uſe of this table will be eaſily underſtood by the following examples.

EXAMPLE I.

Suppoſe a clock goes mean ſolar time when the pendulum vibrates 2° on each ſide of the perpendicular, what will it loſe per day when the pendulum vibrates through an arc of three degrees? Examples.

Againſt 2° and 3° in the firſt column, we have 6,″6 and 14,″8 reſpectively in the ſecond column. Then 14,″8 — 6,″6 = 8,″2 = the time loſt per day.

EXAMPLE II.

Suppoſe a clock gains 1″ per day, when the pendulum vibrates 1° 49′ on each ſide of the perpendicular, what would be its rate when the pendulum vibrates 1° 51′?

Between 1°$\frac{3}{4}$ and 2° in the firſt column, we have ,″107 in the fourth column. Then 1″— ,″$\overline{107 \times 2}$ = 0,″786 = the daily rate of the clock gaining.

I am, with reſpect,

Sir,

Your humble ſervant,

E. WALKER.

ANNOTATION.

The arc of vibration in clocks is likely to be affected in practice by changes in the denſity of the air, in its temperature, and in the action of the firſt mover; and theſe cauſes will moſt probably afford reſults conſiderably different from thoſe inferred from the ſimple pendulum in vacuo. I take it to be experimentally aſcertained by the regiſters of the performance of aſtronomical clocks very firmly fixed, that the gridiron pendulum, with a very flexible ſpring ſuſpenſion, is not ſenſibly affected during the different ſtations of the barometer and thermometer; and I apprehend that the firſt mover, if a weight, muſt operate either with a conſtant force or with periodical variations, occaſioned by the train or mechaniſm through which its action is tranſmitted, the influence of oil, &c. which periods are rendered ſhorter by the well known methods of diſengaging the eſcapement, or parts neareſt the pendulum. Many other conſiderations will offer themſelves to the learned author, as to Whether theory can be applied to an engine ſo compounded as a clock, &c.

thofe parts of the vibration to which the maintaining power may be applied fo as to increafe the arc, while the time may be either lengthened or fhortened according to the circumftances, the general nature of the efcapement, &c. and thefe will probably lead to a conclufion, that the corrections of time to be applied to a clock, which either in its arc of vibration or otherwife gives fymptoms of irregularity, can be gained only from direct obfervation.

W. N.

XIII.

Defcription of Atkins's Hydrometer for afcertaining the fpecific Gravities of fpirituous Liquors. By J. FLETCHER, *Efq. Communicated by the Author.*

Utility of defcriptions of philofophical inftruments.

AMONGST the various papers which are to be found in the works of thofe journalifts who have fo materially contributed to the advancement of our knowledge, there are perhaps but few which have been more effectually conducive to this end than thofe which are appropriated to the defcription of the inftruments of fcience. It is indeed much to be lamented, that the merchant, the manufacturer, and the artizan, fhould continue to be fo generally deprived of the advantages to be obtained from the ufe of fuch of them as are adapted to their purpofes, for want of the neceffary information with regard to the mode of applying them.

The inftrument which is here intended to be defcribed, is one of thofe which ftand in a great meafure in this predicament; and as it appears very well to deferve a defcription, it is conceived that, imperfect as the following one is, it will not be unacceptable.

Spec. gr. only criterion of the ftrength of fpirits.

It is now univerfally acknowledged, that the fpecific gravities of fpirituous liquors afford the only tolerable criterion of their ftrength, and confequently of their comparative values. To afcertain the fpecific gravity of a liquid with a confiderable degree of precifion, is an operation of no great difficulty. The moft accurate method of performing it is perhaps that which is alfo the fimpleft: to weigh the fluid in a veffel in which its bulk is capable of being nicely meafured. Much more, however,

ever, remains to be done in order to the eſtimation of the ſtrengths of ſpirtuous liquors: the contraction of the maſs of a compound of alcohol and water on their mixture, and the variation in its meaſure in reſpect of temperature, are each of them of ſufficient practical importance to render their appreciation neceſſary when the value of the ſpirit is to be diſcovered. Thus for example, 18 gallons each of alcohol and water will produce only 35 gallons of the compound, and a difference of 30° in temperature of Fahrenheit's ſcale, occaſions ſuch a change in the ſpecific gravity of proof ſpirit, as, if omitted to be taken into conſideration, would render the dealer liable to an error of upwards of ten per cent. in the eſtimation of its ſtrength and conſequent value.

Corrections neceſſary to be applied for concentration and temperature.

18 parts each of alc. and water produce only 35 of the mixture. A variation of 30° of temp. occaſions a change of 10 per cent. in the apparent ſtrength of proof-ſpirit.

In commercial tranſactions with reſpect to ſpirituous liquors, it is neceſſary to appreciate their ſtrength by comparing it with that of ſpirit of a certain ſuppoſed quality in this reſpect as a ſtandard. This ſtandard-ſpirit (which is called *proof-ſpirit)* is of ſuch a degree of ſtrength that its ſpecific gravity is about .920 at 60° of Fahrenheit's thermometer; and the object of enquiry in all caſes is with regard to the quantity of this proof ſpirit which would be equivalent to a given quantity of any ſpirit under examination. The language of the ſpirit-dealers with reſpect to their terms of "*over-proof*" and "*under-proof,*" has in all caſes this kind of reference to commercial value. When they ſay that a certain kind of ſpirit is 30 per cent. over proof, they mean that if 100 parts by meaſure be increaſed to 130 by the addition of water, it will become of proof ſtrength; and when they ſay that it is 30 per cent. under proof, they mean that 70 parts by meaſure of proof-ſpirit will become, when increaſed to 100 by the addition of water, of equal ſtrength to that of the ſpirit in queſtion. If therefore a gallon of proof ſpirit be worth 8s. 4d. the ſame quantity of the former kind of ſpirit will be worth 10s. 10d. and of the latter 5s. 10d.

Commercial eſtimation of value of ſpirituous compounds,

by compariſon with proof-ſpirit.

Definition of terms "over-"proof" and "under-proof."

It is not therefore ſufficient for the purchaſer of a ſample of ſpirit, or the officer who is to collect the duties on it, to be informed of its ſpecific gravity at a given temperature: it is neceſſary for him to aſcertain the quantity of proof-ſpirit which is capable of producing, or being produced, by the ſpirit in queſtion by the addition of water only, and which is conſequently equivalent to it in value.

The deſideratum is to find the equivalent quantity of proof-ſpirit.

The

Calculation inapplicable to this purpose.

The ſolution of this problem being almoſt impoſſible by any application of mathematical formulæ, it is of courſe deſirable to be enabled to obtain it inſtrumentally, or by inſpection of tables previouſly laid down from experiment; and the uſe of the latter, under ſuch circumſtances as thoſe in which operations of this kind are generally performed, having been found inconvenient, its inſtrumental ſolution has been univerſally preferred. This has been attempted principally in two ways: by the hydrometer, with a multiplicity of weights adapted to the various corrections for temperature; and by the ſame inſtrument more ſimply conſtructed, ſo as to indicate merely the ſpecific gravity of the liquor; the neceſſary corrections being applied by a ſcale or ſliding-rule.

Inconvenience of tables.

Inſtrumental ſolution preferable.

The hydrometer with many weights,

with a ſliding-rule.

The latter moſt eligible.

The inſtrument here to be ſpoken of is conſtructed on this latter principle, which appears for many reaſons to be the beſt: The hydrometer A B (pl. XV. fig. 3) is of braſs. It is eight inches in length, with an elliptical bulb an inch and a half in diameter, and two inches long. The ſtem is ſquare, each ſide being about $\frac{1}{8}$ of an inch wide, and, when the inſtrument is intended for ſuch liquors only as are ſpecifically lighter than water, is engraved only on one face with the 26 letters of the alphabet, and an O, or zero, at the top and bottom; oppoſite to each, and between every two of which, is a diviſion for marking the point of the ſtem which is cut by the ſurface of any liquor in which it floats, the whole number of diviſions being 55, as ſhewn in the plate. The weight of the inſtrument is about 400 grains, and it is provided with four weights, marked 1, 2, 3, 4, weighing reſpectively 20, 40, 61, and 84 grains, to be applied as occaſion requires on the ſhank of the inſtrument C, on which they are retained by the button or fixed weight B. Theſe weights are ſo adjuſted, that when with any one of them, as for inſtance No. 2, the ſtem of the inſtrument, when floating in a given liquor, juſt emerges to the lower diviſion O, it will, on changing the weight for the next heavier, No. 3, become immerſed exactly to the other diviſion O, towards its ſuperior extremity. The ſtem is by this means virtually extended to five times its real length, and the number of diviſions in effect augmented to 272. Thus without any weight at all, as repreſented in the figure, it would ſink in a liquor whoſe ſpecific gravity was .806, exactly to the upper diviſion O,

Deſcription of Atkins's hydrometer.

Square ſtem, with the letters of the alphabet on one face,

and four weights,

When it emerges to the bulb with one weight, the next heavier ſinks it to the top of the ſtem.

Stem and diviſions thus in effect multiplied by 5.

With the weights ſingly applied, indicates ſpecific gravities from .806 to 1.000;

O, and in one whofe fpecific gravity was .843, to the lower divifion O; the intermediate divifions anfwering to intermediate fpecific quantities. With No. 1, it indicates fpecific gravities from .843 to .880; with No. 2, from .880 to .918; with No. 3, from .918 to .958; and with No. 4, from .958 to 1.000; the furface of water in which the inftrument is made to float with the latter weight, exactly coinciding with the lower O, at 55° of temperature.

The whole interval from .806 to 1.000, comprehending the fpecific gravities of all fpirituous compounds, is in this manner divided into five nearly equal portions, each of which, comprizing from .038 to .040, is meafured by the whole length of the ftem; fo that each of the 54 intervals on its face correfponds to confiderably lefs than an unit in the third place of the fpecific gravity, and indicates a difference of about one half per cent. or two quarts in 100 gallons, in the quality of the compound, and the inftrument is therefore abundantly fenfible to very minute variations in thefe refpects.

Each length of the ftem anfwering to about .04; and each divifion to lefs than .001 in fpec. gr. or ½ per cent. in ftrength.

With regard to the application of the weights, no error can poffibly be committed. If the inftrument floats in any liquor, fo that its furface cuts any part of the ftem, it is properly loaded. Any weight but the proper one will either fink it entirely, or fuffer the ftem to rife totally above the fluid.

Spec. gr. indicated by the divifions on the ftem compared with the rule.

The fpecific gravity indicated by any divifion on the ftem of the hydrometer, is feen inftantly by reference to a fliding-rule belonging to it, whofe two faces are fhewn, plate XV. fig. 2 and 3, on which the lines of divifions A A, A A, marked with the letters of the alphabet, reprefent thofe on the inftrument when loaded with the weight, whofe number is marked over the O at the commencement of each feries, and the exterior lines of divifions B B, B B, near its edges, fhew the correfponding fpecific gravities compared with that of water at 55°.

Correction for temperature applied,

In order, however, to afcertain the ftrength and value of the fpirit, it is neceffary to examine its temperature and apply the proper correction, which is done with extreme accuracy by the fame fliding-rule, by an ingenious application of two fcales of unequal parts to each other, *viz.* the lines C C, C C, marked "*Atkins,*" and the lines A A, A A, marked with the letters of the alphabet. The mode of doing this is as follows: the temperature of the liquor being taken by the thermometer accompanying the inftrument, the afterifk or index, plate XV. fig. 1, on

by fetting an index on the flider to the degree of temperature as fhewn by the thermometer.

on that part of the ſlider oppoſite to the thermometrical ſcale D, is to be ſet to the degree of temperature ſo found, and the diviſion on the line C, marked "*Atkins*," which then correſponds on either ſide of the rule with that letter and diviſion on the alphabet line A, at which the ſtem was cut by the ſurface of the liquor, indicates the ſtrength and value in commercial terms. Thus ſuppoſe the temperature to be 68° (at which the index appears ſet in the plate), and the hydrometer, when loaded with its weight No. 1, to float with the ſurface, cutting the diviſion I on the ſtem (correſponding to a ſpecific gravity of about 854½), we find, on ſetting the ſlider of the rule to 68° on the thermometer ſcale, that 46 over proof correſponds to that letter and diviſion, and is conſequently the ſtrength and value of the liquor.

Diviſion on ſlider anſwering to the letter on the ſtem gives per centage.

Example from the rule ſet as in the plate.

The lines marked "*Dicas*" and "*Clark*" on the rule, are for giving the appreciation of the values and ſtrengths as ſhewn by the inſtruments invented by thoſe artiſts, and are intended for the uſe of ſuch as have been in the habit of employing them; the latter being perhaps inſerted principally on account of the uſe of Clark's inſtrument for the purpoſes of the revenue.

Uſes of the other lines on the ſlider.

The figures on the line marked "*concentration*" amongſt the *over-proofs*, indicate the diminution in volume which takes place on reducing the given compound to proof; and amongſt the *under-proofs*, the diminution which takes place on reducing proof-ſpirit to that ſtrength. This muſt in the firſt caſe be deducted from the quantity marked on Atkins's line (C), in order to obtain the accurate per centage according to the eſtimation of commercial men. For inſtance, 50 over-proof on Atkins's line indicates, that if to 100 parts of ſpirit of the ſtrength which anſwers to this diviſion, we add 50 of water, we ſhall get proof-ſpirit, of which, however, the quantity will be only 147 parts; the concentration or diminution of bulk by mixture being 3 parts. With regard to the ſpirit which is under-proof, the figures on the concentration line indicate the diminution of volume in pints for every 100 gallons of the mixture: if, for inſtance, 70 gallons of proof-ſpirit be combined with 30 of water, we get a compound whoſe ſtrength is that which is marked on the line C, 30 under proof; but the concentration being ſeven pints, we ſhall get only 99⅛ gallons.

Of the concentration-line.

Concentration to be deducted from the per centage.

Example. Diminution of 3 parts on adding 50 of water to 100 of 50 over-proof.

Concentration on mixture of proof-ſpirit and water.

4 When

When the hydrometer is intended only for ſpirituous compounds, the weights are applied ſingly as before-mentioned: if, however, in addition to the weight No. 4, the others are ſucceſſively applied, it becomes applicable to the examination of worts and other liquors, whoſe ſpecific gravities are from 1.000 to 1.109, or, in the language of the brewery, up to 40 lbs. per barrel heavier than water. In this caſe the other three ſides of the ſtem are alſo graduated, and another rule with an ivory ſlider, carrying a thermometrical ſcale for comparing worts at different temperatures, is included in the caſe with the inſtrument.

Applicable to worts by applying the weights doubly.

Spec. gr. of worts deſignated by lbs. per barrel.

Additional rule for the brewery.

The writer of this paper having made a conſiderable number of experiments with this inſtrument, on the ſpecific gravities of a variety of ſpirituous liquors, had originally intended to have given their reſults; but it has already run to ſuch a length, that they muſt be deferred to a future communication. The errors, even including thoſe which muſt neceſſarily ariſe from the various temperatures of the compounds, and the different quantities of foreign matters with which theſe fluids, in an impure ſtate, may be ſuppoſed to be charged, appear to fall within very narrow limits; and the extreme facility and expedition with which it reſolves the queſtions to the ſolution of which it is applicable, cannot fail to render it very highly valuable to thoſe for whoſe purpoſes it is principally intended.

Errors in reſults very ſmall, and advantage of facility of uſe very conſiderable.

XIV.

An Examination of Sig. VOLTA'S *Experiments which he calls fundamental, and upon which his Theory of Galvaniſm reſts; with a Deſcription of a very ſenſible Electrical Condenſer, and an Explanation of the Action of the Electric Fluid in the Galvanic Inſtrument. By* JOHN CUTHBERTSON, *Philoſophical Inſtrument Maker, No, 54, Poland Street, London. Communicated by the Author.*

IN Vol. I. 8vo, page 136, paragraph three, of this Journal, Sig. Volta affirms, "If two inſulated diſcs, one of copper and "the other zinc, be applied together for a moment and then "ſeparated, the zinc will be poſitive and copper negative." I have always had the ſame reſult, but ſome times much weaker than at others.

Volta's expt. of the contact of inſulated diſcs of copper and zinc producing + el. in the zinc, and — el. in the copper. Succeeds.

Page

Expt. of copper and zinc joined, and the copper to touch a zinc condenfer, and afford — el. in this laft.

Page 137, in the laft paragraph, it is faid, " If a piece of " zinc to which is joined a piece of copper, and the zinc held " by two fingers or in any other manner, and the copper made " to touch the fuperior difc of the condenfer, which is zinc, " while the inferior is in communication with the ground, a " moment afterwards raifing the upper difc in the air, it will " be negatively electrified." This I have always found the contrary, *i. e. pofitive*.

Did not produce — el. but + el.

Expt. of zinc touching a copper condenfer, and affording no el.

Page 138, paragraph three, it is alfo faid, " If the fuperior " difc of the condenfer be copper, and a piece of zinc be made " to touch it immediately without any intervening fubftance, " nothing will be obtained, becaufe the zinc being then in " contact at the two oppofite ends with copper and copper, " two equal forces act in oppofite directions, and by that " means deftroy or counterbalance each other." If the fuperior difc was copper, I always found it negative, and if it was zinc touched by a piece of copper, it was pofitive; both contrary to Sig. Volta's affertion.

Did not fucceed.

We find then that only one experiment out of three fucceeds, which he calls fundamental, upon which his theory of galvanifm is founded.

The foregoing expts. were often repeated.

Knowing Sig. Volta's abilities both as a philofopher and experimentalift, I own I mention the refult of the foregoing experiments (though it is after numerous repetitions) with much diffidence; but the experiments are fo fimple, that it is almoft impoffible that I can have erred.

By reafoning upon the phenomena, when copper and zinc are made to touch each other, it becomes eafily explicable by the old known laws of electricity.

EXPLANATION.

Explanation. In expt. I. either the zinc *attracted* or the copper *repelled* the elect. while in contact.

In the firft experiment, where zinc and copper are made to touch each other, we find, on feparation, that the zinc has acquired a greater fhare of electric fluid than it had before the touch, and the copper lefs, by virtue of their mutual action upon each other when in contact. So that the zinc muft have either attracted the electric fluid out of the copper, or the copper muft have repelled it from itfelf into the zinc; and that the firft is the true caufe may be proved by connecting the copper difc with conductors, and then touching it by the infulated zinc, which will be found, on feparation, to be much more ftrongly pofitive

positive than when the copper was insulated. For whatever quantity of electric fluid may be drawn out of the copper by the zinc, is recovered by its being in connection with continued conductors; whereas, if it had been repelled from the copper into the zinc, it must be stronger when insulated, because when insulated, the copper has no other body but the zinc to receive that which it repels from it; and when not insulated, every other conductor with which it is connected takes a part in proportion to their conducting property. As the zinc disc is found to be much stronger when the copper disc is not insulated, I conclude that zinc has the property of attracting the electric fluid out of copper when they are in contact.

The effect is stronger when the zinc alone is insulated, and less when the copper alone is insulated; hence the zinc is inferred to *attract*.

In regard to the second experiment, where the zinc is found to be much more strongly positive than in the first, is clear from what I have already said, *i. e.* because the copper disc is not insulated, but is free to act as their mutual action upon each other may require.

In expt. II. the insulation of the zinc increases the effect.

In the third experiment, in which no effect should follow (according to Sig. Volta), the zinc was found to be as strongly electrified as in the last experiment. Neither the zinc nor the copper being completely insulated, they are at liberty to act upon each other as their mutual contact directs; the zinc at liberty to attract and the copper to give, which is in no wise repugnant to the old laws of electricity.

Expt. III. The metals being at liberty to assume the respective states on contact, did so accordingly.

How Sig. Volta could be led to such erroneous conclusions is not easy to understand, unless he was deceived by the condensers he made use of. "To render that feeble electricity "sensible and manifest, he recommends flat metallic plates co-"vered with a slight layer of sealing wax or lac varnish," which I was not a little surprised at, because I have always found such condensers very equivocal in their results, and shew different signs without any variation or obvious cause, or at least such as I was unable to detect. They are more easily excited by a negative power than by a positive one, and retain it much more tenaciously; and hence, when they happen by any means to be strongly electrified by a negative power, it is almost impossible to discharge them of it, so as to be fit to proceed on immediately with experiments that require nice investigation.

Sig. Volta supposed to have been deceived by his *varnished condensers*.

Since the invention of the galvanic instrument, various electrometers, condensers, doublers, multipliers, &c. &c. have been used to investigate its electrical properties, all of which appear

Mr. John Read's construction of the condenser.

appear to me to be much inferior to one conftructed in the year 1796, by Mr. John Read, an ingenious electrician at Knightfbridge; but as he invented it near the time of his retiring from bufinefs, he did not publifh it, confequently it is known but by very few electricians. I have found it of great ufe not only in galvanifm, but in all experiments wherein fmall quantities of electricity are to be made evident; and I doubt not but that it will be confidered as a valuable acquifition by electricians. I have found it capable of rendering much fmaller quantities fenfible than any other inftrument. By this inftrument we are able to learn the pofitive and negative fides of only one piece of *zinc, copper, and wet cloth;* and I have not heard that it has been done by any other inftrument lefs than with a feries of twenty*.

A Defcription and Ufe of Mr. Read's compound Electrical Condenfer.

Defcription of Read's condenfer.

Fig. 1. plate XVI. reprefents a vertical fection of the large condenfer: *a a* is a round flat plate of brafs of about eight inches, ftanding infulated upon a wooden foot *g; g h* is a hollow brafs cylinder, and *f h* is a folid ftick of glafs, with a brafs mounting at *f*, to which the plate *a a* is fixed; *b b* is another plate of brafs fomewhat lefs in diameter, and has a round hole in the middle of about two inches diameter; *c d* is a hollow cone fixed to *b b* at *c c; d e* is a hollow brafs cylinder fixed to the cone at *d*, and made to flide up and down upon the cylinder *g h; i* is a milled head-fcrew which holds *b b* faft when it is at its proper diftance from *a a*, where a ftop is made for that purpofe; when *i* is loofe, *b b* falls down by its own weight, and refts upon the foot *g*.

Altered by the author.

The above is the original conftruction, which I found more complex and lefs portable than I could wifh; for which reafons I make them in the following manner; and fixing the condenfing plates in a vertical pofition inftead of horizontal, it has all the advantages, is more fimple, and perfectly portable, and I have no doubt will meet with the approbation of Mr. Read himfelf.

Defcription of the improved Condenfer.

Defcription of the improved condenfer.

Fig. 2 reprefents a vertical fection of the large condenfer ftanding edgewife to the eye: *a a* and *b b* are two flat round brafs

* Bennet's electrometer alone fhewed the ftate of 40 pairs. See Van Marum in Philof. Journal, octavo, I. 174.—N.

braſs plates of about ſix inches; *c* is a glaſs cylinder fixed at one end into a wooden foot *d; e* is a braſs ball, and mounting at the other end to which the plate *b b* is ſcrewed faſt; *f* is a braſs wire with a joint at its lower end, and at the other end a ball to which the plate *a a* is ſcrewed, ſtanding parallel to *b b.* By means of the joint, the plate *a a* may be moved back in the ſituation of the dotted outline *a g*. The joint has a ſhoulder which ſtops the plate *a a,* and keeps it at its proper diſtance from *b b*.

Bennet's electrometer with a condenſer.

Fig. 3 is the common gold-leaf electrometer, with an addition of two vertical braſs plates of about 1½ inch in diameter; one is ſcrewed faſt to the braſs mounting at the top of the electrometer, and the other to a wire which has a joint fixed to the foot of the electrometer, by which it is moveable, and is ſet either parallel and at a proper diſtance from the other plate, the joint being furniſhed with a ſtop for that purpoſe, or may be moved backwards in the direction of *g a,* fig. 2: *l* is a braſs cup with a ſhank at the bottom, which ſcrews into a hole in the top of *e,* fig. 2, ſerving to examine the ſtate of electricity excited by dropping of metals, &c.; *m* is a ſtick covered with tinfoil, which ſcrews into the hole at *e,* to examine the electricity of the atmoſphere; *n* is a braſs wire jointed at *o,* with a ſhank, which can alſo be ſcrewed into *e* when required, or into the top of the gold leaf electrometer, fig. 3. Theſe two inſtruments, fig. 2 and 3, are uſed ſeparately or combined, as the nature of the experiment may require. When the experiment requires the aid of both condenſers, they are combined, as is repreſented fig. 4, the two fixed plates ſtanding towards the eye. The fixed plate of the large condenſer has a ſmall braſs pin at one ſide, which, when the inſtruments are uſed together, muſt touch the fixed condenſing plate of the gold-leaf electrometer *.

THE METHOD OF USING THE DOUBLE ELECTRICAL CONDENSER.

To ſhew the Electric Fluid excited by Efferveſcence, &c.

Uſe of the two condenſers. To ſhew the electricity from efferveſcence, &c.

Screw *l* into the top of the ball *e* of the large condenſer, and ſet therein a china or glaſs cup with proper ingredients for that purpoſe;

* Inſtead of gold leaf Mr. Read uſes very fine fibres of flax, which he thinks more ſenſible than gold leaf; but they are very difficult to be ſeen, and more eaſily deranged. If gold leaf be properly managed, I think it preferable.—C.

purpose; then join the two instruments as in fig. 4. While the effervescence is going on, turn back the moveable plate of the large condenser into the position of the dotted line, fig. 2, taking care not to touch the fixed plate; then if the excited electricity be very strong, the gold leaf will diverge; but if not, just move the electrometer so that it is quite free from the pin, turn back its moveable plate, and if a sufficient quantity of electric fluid be excited, the gold leaf will diverge.

To shew if there be any sensible Quantity of Electric Fluid in the Atmosphere.

Atmospheric electricity.

Screw the stick *m* into *e* of the large condenser; a convenient place being chosen not much surrounded by buildings or trees, set the instruments combined as in the last experiment, and proceed in the same manner.

Method of applying the combined Condensers to the Galvanic Instrument.

Method of applying the condenser to the galvanic instrument.

Screw the short end of *n o* into *e* of the large condensing plate (the instruments being combined as in the former experiments); bend the end downwards, at such a distance from the table, or whatever it may stand upon, that the two pieces of metal, zinc and copper, as at *n*, can be put under it, and drawn away from under it again, without its touching the table when the metal is drawn away. Take two pieces of metal, zinc and copper, about the size of half a crown or upwards, either separate or soldered together, with their flat sides in contact, and push them under the end *n* of *o n*. After remaining a short space of time, a quarter or half a minute, draw them away from under the point, and take notice that the point does not touch the table, or any other conductor; then turn back the moveable plate of the large condenser; move the electrometer so that its plate shall no longer touch the pin of the large plate, and then turn its moveable plate back; the gold leaf will remain undisturbed.

Electricity from the galvanism of a single pair of plates.

Turn up the condensing plates to their first position; place the two instruments together as before, taking particular care that the fixed plate of the electrometer condenser touches the pin proceeding from the large plate. Lay upon the pieces of metal before used, a piece of woollen cloth well soaked in a solution of muriate of ammonia, or any other menstruum com-

monly

monly uſed for the galvanic experiments either upon the zinc or the copper, and puſh them under the point of n again. Preſs the point down upon them, that it may be perfectly in contact; after they have remained the time before-mentioned, draw the metals away, and ſeparate or turn back the large condenſer plate, and alſo the ſmall one, after ſeparating it from the pin of the large one, and immediately the gold leaf will diverge. If the zinc was the uppermoſt, then the gold leaf will diverge with poſitive electricity; but if it was underneath, the gold leaf will diverge with negative electricity. It makes no difference in the general effect, upon which metal the wet cloth was laid; or whether two pieces of cloth were uſed, one under the metals and the other above; or only one either above the metals or under them. But if the cloth be only laid upon the copper and not upon the zinc, the electric fluid brought into action will be ſo weak, that the combined inſtrument can hardly ſhew it; if laid upon zinc, the divergency will be about $\frac{1}{40}$ of an inch; ſometimes more and ſometimes leſs *.

By reflecting on this phenomenon I found the following explanation, without having recourſe to any new hypotheſis.

Explanation of the Action of the Electric Fluid in the Galvanic

Explanation of the facts offered. Zinc and copper in contact arrive at an equilibrium of electricity; the zinc + and the copper —.

When flat pieces of zinc and copper are laid in contact, the zinc becomes poſitive and the copper negative at the moment of the touch; and while they remain in contact, the electric fluid contained in them both is perfectly in equilibrio. The copper has given and the zinc has received ſuch a quantity of electric fluid as their mutual action upon each other required; and in conſequence of this property, they preſent a mutual reſiſtance to any further change being produced upon them. If then any menſtruum be added to the oppoſite ſide of the metals, capable of producing a change in their metallic property (ſuch as the fluid contained in the wet cloth), a change in their electrical property muſt of courſe follow. But as this change in the metallic property is only ſuperficial, it will only be there that its electric property is changed. The other parts of the two metals in contact will remain unaltered, and maintain their property of reſiſtance. The change produced by the action of

Chemical action alters the electrical property of that part of the metal in which it takes place.

* When the atmoſphere is in a favourable ſtate. In this as well as all other experiments where ſo ſmall quantities are to be made evident, the atmoſphere has great influence.

the

This change *is aſſerted to be* of an oppoſite nature to the electric ſtate of the unaltered metal, and therefore produces electric ſtates at the wet ſurfaces oppoſite to thoſe of the dry metals: which do not paſs from zinc to copper *through the metals* and become quieſcent; but from zinc to copper *through the fluid*, this paſſage being eaſieſt. Hence the galvanic current: which is progreſſive, becauſe the fluid cannot perfectly conduct the electricity as faſt as it is extricated.

the menſtruum, with reſpect to its electrical property upon that part only of the metal whereon it has acted, is exactly the reverſe of that of the parts not acted upon. The part of the zinc thus acted upon, muſt conſequently be diſpoſed to throw off its electric fluid, and would give to that part of the copper, which by a like action is diſpoſed to abſorb it) ſo that the two ſtates of the ſurfaces acted upon would unite and counterbalance each other imperceptibly) if it were not oppoſed by the ſtill exiſting property and mutual action of the two metals in their parts not acted upon by the menſtruum; hence it follows that the electric fluid is propelled forwards from the zinc *through the menſtruum* to the next adjoining copper in the pile or trough; but this can only happen in a progreſſive manner, on account of the menſtruum being an imperfect conductor, which appears to be an indiſpenſible condition to the maintaining any electric intenſity.

The ſhock from the accumulation ſuddenly tranſmitted.

The ſhock or ſenſation felt on touching the two oppoſite ends of the galvanic inſtrument, depends upon the menſtruum, together with the reſiſting property of the two metals in contact. The fluid muſt be adequately proportioned between being an electric and a perfect conductor. If it were a perfect conductor, the electric fluid would paſs from the zinc through the menſtruum to the next adjoining copper, as quick as it is given off by the altered part of the zinc; no accumulation would enſue, and conſequently no ſenſation of ſhock or diſcharge would be perceived. If it were an electric, the electric fluid given off by the altered zinc would be ſtopped, and accumulate till it become poſſeſſed of ſufficient force to overcome the mutual reſiſtance of the two metals in contact, and paſs through by a reverſe motion to the copper; conſequently no ſenſation would be felt by forming a communication between the two oppoſite ends. For though there would be an accumulation, yet it cannot be united ſo as to act in concert with that of the other combined metals; being ſhut off by the interpoſed electric menſtruum, and too feeble of itſelf to affect our ſenſes.

ANNOTATION. W. N.

Short hiſtory of inſtruments for ſhewing minute quantities of electricity. W. N.

At page 396, vol. I. of the quarto ſeries of this Journal, an outline is given of the hiſtory of all the inſtruments for ſhewing or meaſuring minute quantities of electricity, with a ſtatement of their advantages and defects. With regard to the condenſer diſcovered

discovered by Volta in 1780 *, and used for atmospheric electricity and in the grand discovery of the electricity produced by chemical changes, it was applied very early by Cavallo in the compound form; that is to say, by charging a smaller instrument with the contents of a larger. Mr. Bennet before 1787 †, added the condenser to his electrometer, and invented the method called doubling, which had before been used with the electrophore by Lichtenberg ‡ and Klinoock §. Our electricians, particularly Mr. Cavallo and Mr. Bennet, were at this time fully aware of the spontaneous electricity of the doubler, and the evils to be apprehended from the varnish and the actual contacts. I think it was Mr. Cavallo who substituted small knobs of sealing wax instead of varnish, which I saw adopted in a mechanical doubler by Dr. Darwin in 1787; and in the year 1788 I communicated to the Royal Society || the revolving doubler, in which the plates approach and recede without touching; and soon afterwards I made the spinning instrument, consisting of a condenser and an electrical well. In the same year Mr. Cavallo published ** his *collector* of electricity, which is a condenser having two uninsulated plates, which separate like plate *a*, fig. 2, on each side of *b*, without contact; and in the third vol. of his "Complete Treatise on "Electricity," 1795, he describes his *multiplier*, which consists of two condensers, equally perfect; one of which is made to charge the other by repeated alternations of the compensating plate.

The above short sketch will enable the reader to ascertain to what extent the ingenious contrivers of the instruments in plate XV. have availed themselves of the labours of former operators.

* Journal de Physique for May and August 1783, and Phil. Transf. LXXII. p. 237.

† Phil. Transf. LXXII. p. 32.

‡ Journal de Phys. Jan. 1780, p. 20.

§ Phil. Transf. LXVIII. p. 1029.

|| Phil. Transf. LXXVIII.

** Phil. Transf. LXXVIII.

XV.

Observations on the Phosphorescence of the Tremolite, and of the calcareous Phosphate of slow Solution, known by the Name of Dolomie. By M. LE COMTE DE BOURNON, *Fellow of the Royal and Linnæan Societies. Translated from the Original; communicated by the Author.*

The phosphorescence of minerals has been little inquired into.

FEW researches have yet been made concerning the phosphorescence of the bodies of the mineral kingdom. No satisfactory explanation has yet been given of this phenomenon, though this knowledge would undoubtedly throw new light upon the study of minerals, and prove a great acquisition to natural philosophy and chemistry.

The methods of exacting it: 1. by friction; 2. by heat.

This property of emitting light, which daily observation shews to belong to many more minerals than had formerly been suspected, requires particular management. In some fossils, such as quartz, blende, corundum, &c. &c. it becomes sensible only by friction. In others it exhibits itself only when the mineral is placed upon a red hot coal, or upon any other body heated to a similar temperature. This is the case with the carbonate of strontian, of barytes, &c. In others again, the phosphorescence is developed both by friction and by heat, as is the case with the phosphates and fluates of lime, as well as with a great number of carbonates of the same earth, particularly those of a brown or yellowish colour.

These facts give occasionfor several questions, the solution of which would be extremely interesting.

Does it arise from combined light?

Do these two kinds of phosphorescence depend upon the same cause? In all the stones which exhibit them, and are at the same time coloured, the colour diminishes in proportion to the disengagement of the phosphorescence by the action of heat: and when they cease to be phosphorescent, they at the same time intirely cease to be coloured. Does this phenomenon proceed from the disengagement of the combined or interposed light? Does the colour in these stones always belong, in reality, to metallic oxides, particularly those of iron? May it not rather belong simply to the combined light? In this case, may it not be supposed that the light is decomposed, by combining with these stones, and that it then entered into their

compofition only by infulated rays, or by combination of two or more, and not the whole of the rays? We are, however, enabled to make an obfervation with refpect to this fubject, namely, that thefe ftones conftantly exhibit by the action of heat, a phofphorefcence of the fame colour, whatever may be the colour of their proper fubftance. For inftance, the fluates of lime which exhibit the moft lively and variegated colours, conftantly give a light inclining to the violet, with the fingle exception of the Siberian variety, which has been named the Chlorophane, and which, though of a violet colour, gives a phofphorefcence of a beautiful emerald green. In others, as in fome carbonates of lime, in thofe of barytes, of ftrontian, &c. though thefe ftones are perfectly colourlefs, the phofphorefcence is conftantly reddifh, or orange yellow. What may be the caufe of thefe contrafts?

The phofphoric colour is the fame in all ftones of the fame fpecies, however themfelves may be coloured.

In fome cafes the caufe which produces the phofphorefcence of the ftones feems to belong to an effential part of their fubftance, which is never completely expelled from them. This is the cafe with the calcareous fluates and phofphates, &c. In others it appears to be purely accidental in the ftone, and fhews itfelf only in a certain number of individuals belonging to the fame fpecies. In the firft cafe, this property ought to be indicated amongft the fpecific characters of the ftone; in the fecond, it cannot ferve as character of the fpecies, but can merely be ufed to defignate one of its varieties. Such is, for example, that which exifts in the tremolite and in the dolomie, refpecting which I intend here to offer fome obfervations, which appear to me to deferve the attention of mineralogifts.

It may arife from a permanent caufe, or from fome portion that can be abftracted.

M. de Sauffure and Profeffor Blumenbach were, as far as I know, the firft who obferved the two kinds of phofphorefcence in the tremolite; and, fince them, all the works of mineralogy have placed this property amongft the diftinctive characters of this ftone. Many tremolites, indeed, are endowed with this double phofphorefcence; but this is by no means the cafe with all, nor can this character be confidered as effential to its nature.

The tremolite is not in all fpecimens phofphorefcent.

The tremolite, both that which is found in different vallies of Mount St. Gothard, and that which is brought to us from a great number of other places, is generally inclofed in a granulated carbonate of lime, the grains of which are of various degrees of finenefs, and their adhefion more or lefs confiderable.

Thofe which are in a phofphorefcent gangue are themfelves phofphorefcent; and on the contrary when the gangue is not phofpho-

Among

reſcent the tremolite is not ſo.

Among theſe carbonates of lime, which conſtitute the gangue of the tremolite, and very frequently belong to the ſpecies which is called dolomie, a great number are endowed with the double phoſphoreſcence; but, on the other hand, we meet with ſeveral, in which not the ſlighteſt trace of this property can be found. The tremolites incloſed in the firſt partake, though in rather an inferior degree, of their phoſphoreſcence, whereas thoſe which are incloſed in a non-phoſphoreſcent carbonate of lime are equally void of that property.

That the phoſphoreſcence aroſe from interpoſed carbonate of lime, was proved by taking it up by nitric acid, which deſtroyed the luminous quality.

From the very firſt moment when I obſerved this fact, it occurred to me, that the phoſphoreſcence of the tremolite might very probably proceed merely from that of the carbonate of lime, which undoubtedly is interpoſed between its parts. I therefore ſelected ſome cryſtals that were incloſed in phoſphoreſcent carbonate of lime, and after having ſatisfied myſelf that they were themſelves phoſphoreſcent, both by friction and by the action of heat, I digeſted them for ſome time in nitric acid. When I took them out, their ſurface was perforated with ſmall cavities, occaſioned by the ſolution of the portions of carbonate of lime, and friction was then incapable of making them diſplay the ſlighteſt phoſphoreſcent light. This light, however, was emitted, at the point immediately after ignition, though in an extremely weakened degree. I afterwards reduced ſome of the ſame cryſtals to a coarſe powder, and this powder having remained again for ſome time in the acid, was intirely deprived of all its phoſphoreſcent property.

This quality therefore belongs to the variety, and not the ſpecies.

I could no longer doubt that the carbonate of lime, interpoſed between the particles of the tremolite was the real cauſe of the phoſphoreſcence of this ſubſtance, when its gangue was endowed with the ſame property. It therefore appeared to me to be at the ſame time perfectly proved, that phoſphoreſcence cannot be ranked amongſt the characters of the ſpecies in this ſubſtance, and that it ought to be conſidered merely as diſtinctive of one of its varieties.

Whether the large portion of lime ſaid to exiſt in tremolite can be admitted *as a conſtituent part?*

A new doubt, which was a very natural conſequence of this obſervation, now preſented itſelf. Could it be true, that the lime which chemiſts have reckoned to amount to $\frac{18}{100}$ amongſt the conſtituent parts of the tremolite, does exiſt in it in ſo large a proportion. In order, if poſſible, to ſatisfy this doubt, I ſelected ſome cryſtals of non-phoſphoreſcent tremolite, which

had only an argillo-martial ſubſtance for their gangue, and requeſted Mr. Chenevix, who has already rendered ſuch uſeful ſervices to mineralogy, to examine them by analyſis. I likewiſe gave him ſome cryſtals from amongſt thoſe phoſphoreſcent ones which I had broken, and afterwards deprived of their phoſphoreſcence, by digeſting them for ſome time in nitric acid.

My ſuſpicions were verified: Mr. Chenevix found only $\frac{4}{100}$ of lime in each of theſe two analyſes. But what at the ſame time ſtruck me, was that the tremolite taken from the phoſphoreſcent variety, having for its gangue a carbonate of lime of the ſpecies called the dolomie, which is likewiſe phoſphoreſcent, but whoſe calcareous part had been taken away by the nitric acid, gave by analyſis only $\frac{4}{100}$ of argil, whilſt that which was taken from an abſolutely argillaceous gangue gave $\frac{14}{100}$ of the ſame earth. Mr. Klaproth having found no argil at all in the analyſis which he had before made of this ſubſtance, it is probable that its exiſtence in it, like that of the calcareous carbonate, proceeds merely from its ſimple interpoſition.

Probability that a portion of the gangue, whether lime or argil, is *interſperſed* in the mineral.

Theſe two obſervations appeared to me to be very intereſting to the ſtudy of mineralogy, eſpecially to that part of it which relates to analyſis; ſince it ſhews with what care the chemiſt ought to avoid confounding, with the true conſtituent parts of a ſubſtance, thoſe which are foreign to it, and only interpoſed between its parts. It is extremely common to find the parts of a mineral, even in the ſtate of cryſtallization, envelope more or leſs of the portions of that ſubſtance which conſtitutes its gangue; and what may likewiſe contribute much to miſtakes in this reſpect, is the kind of conſtancy with which, (whatever may be its cauſe, has hitherto been little attended to) the ſame ſubſtance, placed in ſimilar circumſtances, admits this interpoſed extraneous ſubſtance, in equal, or nearly equal proportions. It is not therefore ſufficient that the chemiſt ſhould ſelect for his analyſis, amongſt the cryſtals of a ſubſtance, thoſe which appear to him to be the moſt pure, (and the perfection of their form and tranſparency is the ſtrongeſt preſumption which he can have in this reſpect;) but he ſhould alſo repeat the ſame analytical proceſs upon the ſame ſubſtance taken from totally different gangues.

Hence it is of importance that chemical analyſes of any mineral ſhould be repeated upon ſpecimens taken from different gangues.

When

The phoſphoreſcence of tremolite being admitted to ariſe from interſperſed carbonate of lime, will account for the differing obſervations of authors.

When it is once admitted that the tremolite is phoſphoreſcent only in proportion as this property is contained in the carbonate of lime interpoſed in its ſubſtance, the variations which ſome authors have found in its phoſphoreſcence, will be eaſily accounted for. It muſt, for example, be the more eaſily obtained by friction, as the hardneſs of this ſtone is leſs conſiderable; becauſe the friction, by breaking its ſurface, will ſucceſſively arrive at the interpoſed particles of carbonate of lime. And hence it is quite natural that the fibrous varieties ſhould be more phoſphoreſcent than the others, and that theſe ſhould be the leſs ſo in proportion as they are harder.

Though the phoſphoreſcence of tremolite cannot be admitted as ſpecific, yet the facility with which this very hard mineral is cruſhed by preſſure with the hammer and its elaſtic reſiſtance, are peculiar ſpecific characters.

As this character, which was repreſented as ſpecific in the tremolite, muſt no longer be conſidered as ſuch, there is one which has been overlooked, and which, in my opinion, ought to be added to thoſe which have been already obſerved in it; I mean the great facility with which, notwithſtanding its hardneſs, which, in the pureſt ſpecimens is ſuch that it eaſily cuts glaſs, it is cruſhed by the mere preſſure of the hammer, and the kind of flexibility which it then exhibits. If, in breaking it, the preſſure be moderate, the cryſtals of tremolite divide, pretty generally, according to the length of their priſms, into ſmall fibres, which are frequently as fine as thoſe of the amianthus, to which, in this ſtate, they have much reſemblance. We may then even increaſe the preſſure without breaking the fibres, which in this caſe afford by their reſiſtance the ſame ſenſation as is felt from a ſlightly elaſtic body. This effect, as well as the reduction of the tremolites into ſmall fibres, is more diſtinct the leſs pure the mineral. Both properties however are obſerved, but in a much leſs degree in the pureſt tremolites, and conſequently in thoſe whoſe hardneſs is the moſt conſiderable.

Tremolite is found in Scotland, at Mount Veſuvius, and in Bengal.

With a view to add as much as poſſible to the knowledge already acquired reſpecting this ſubſtance, I ſhall add to the liſt of places which have been indicated as the native countries of the tremolite, Scotland, Mount Veſuvius, and Bengal. Mr. Greville's rich cabinet in London contains ſpecimens from each of thoſe different places; a deſcription of theſe will probably be acceptable to mineralogiſts.

Deſcription of the tremolite of Scotland.

The tremolite of Scotland is in the fibrous ſtate, its fibres being very fine and cloſe, part of which is diſpoſed in divergent

gent rays, tranfverfely croffed by other fibres, fo as to reprefent a kind of texture, as is fometimes the cafe with the zeolite mezotipe. This tremolite is of a greenifh white colour: it adheres to a granulated but very compact mafs of carbonate of lime, mixed in almoft equal proportions with the fame tremolite, alfo in a granulated ftate, on which account it ftrikes fire with fteel. This carbonate of lime poffeffes both kinds of phofphorefcence, but the light which it gives is of a flightly blueifh colour; the fame is the cafe with the tremolite which it contains. This carbonate does not belong to the dolomie, and it diffolves in acids in the fame manner as the common carbonate of lime.

The tremolite of Vefuvius is likewife in the fibrous ftate, with fine and clofe fibres; its colour is a greyifh white, it adheres to a gangue compofed of carbonate of lime, of an immenfe quantity of very fine fmall fibres of the fame tremolite, and of a great number of very fmall cryftals of pyroxene, of a beautiful green colour and tranfparent. Some portions of idocrafe are alfo obfervable, of which there is a group at one of the extremities of the fpecimen in pretty large cryftals. The carbonate of lime does not belong to that of flow folution; it poffeffes both kinds of phofphorefcence, and the light which it emits is of a very lively deep orange colour. The tremolite exhibits abfolutely the fame phofphorefcence. Tremolite of Mount Vefuvius.

We owe our knowledge of the tremolite of Bengal to Sir John Murray. It is in pretty large cryftals of a greenifh grey colour, bedded feparately in a granulated carbonate of lime, the very fine grains of which have a ftrong adhefion to each other; a character which, joined to the great whitenefs of this ftone, caufes it very much to refemble a piece of double refined fugar. This carbonate of lime belongs to the fpecies of the dolomie; it is even one of thofe in which I have obferved the folution to be the floweft and moft infenfible, but it is neverthelefs completely diffolved in the nitric acid, leaving only a light whitifh and clouded refidue, which difappears when the acid is diluted with water. Its hardnefs, which is much fuperior to that of the ordinary carbonate of lime, is rather inferior to that of the fluate of the fame earth; and this is the cafe with all the dolomies, not excepting thofe whofe grains have the leaft adhefion with each other. This dolomie is not phofpho- Tremolite of Bengal.

phoſphoreſcent either by friction or by the action of heat, and the tremolite which it contains is intirely in the ſame ſtate. There are many other dolomies alſo which poſſeſs no phoſphoreſcence, as has been alſo obſerved by the Abbé Hauy. This character therefore ſtill belongs to the variety, and not to the ſpecies.

XVI.

Outline of the Hiſtory of Galvaniſm: with a Theory of the Action of the Galvanic Apparatus. By JOHN BOSTOCK, *M. D. From the Author.*

To Mr. NICHOLSON.

SIR,

Introductory letter.

HAVING been lately employed in ſome experimental inquiries on the ſubject of galvaniſm, I found it commodious to arrange the numerous diſcoveries that have been made in this department of ſcience into the hiſtorical form. The facts which have been ſucceſſively developed begin to aſſume ſo elevated a rank among the branches of natural philoſophy, that a ſketch of the moſt important and beſt eſtabliſhed amongſt them ſeems deſirable, in order that the experimenter may be eaſily enabled to ſee what has been done by his predeceſſors, and may thus be prevented from waſting his time and exertions upon points which have been previouſly inveſtigated. From reflecting upon the labours of others, and comparing them with my own experiments, I have been led to form a theory of the action of the galvanic pile, which ſeems to explain in an eaſy manner moſt of its ſingular properties. I am indeed well aware of the undue attachment which every one feels for the offspring of his own imagination, and I ſhall not be ſurprized if you perceive in the hypotheſis many blemiſhes which have eſcaped my notice. I have however ſent you both the hiſtory and the theory, in order that you may inſert them in your Journal, if you think them deſerving of a place there.

I am, SIR,

Your obedient Servant,

JOHN BOSTOCK, *M. D.*

Liverpool, June 1, 1802.

HISTORIC

HISTORIC SKETCH OF GALVANISM.

First publication on ga vanifm by *Galvani* in 1791. Legs of a frog, feparated from the body, were convulfed by extremely minute portions of electricity.

THE firft publication * upon the fubject is the work of Galvani himfelf, which appeared in 1791. It begins by giving an account of the following accidental difcovery. A frog had its hinder legs feparated from its body, except that the crural nerves were left undivided, and was by chance laid upon a table on which ftood an electrical machine. It was obferved that when the animal was placed in contact with, or fufficiently near to, an extenfive furface of a conductor of the electric fluid, if a fpark were taken from the machine in any direction, the legs of the animal were fpafmodically contracted. When the frog formed part of the electric circle, fo that the fluid paffed immediately through it, a quantity almoft imperceptible was found to excite contractions of the mufcles. A much fmaller quantity of the electric fluid produced the effect, when the animal was prepared in the manner defcribed above, than when the body was left intire, becaufe in the former cafe the fluid was confined to the narrow track of the nerves in its paffage along the circuit. A *prepared frog* appears therefore to be a moft delicate electrometer, as it exhibited contractions where no marks of electricity could be difcovered by the inftruments either of Bennet or Cavallo. Galvani afterwards found that contractions could be produced in the limbs of prepared frogs by the electricity of the atmofphere, and it was in confequence of fome arrangements which he made for this purpofe, that he was led to his great difcovery of animal electricity. He found that he was able to produce contractions in the limbs of frogs without the aid of any foreign or artificial excitement, merely by the application of a conducting fubftance from the nerve to the mufcle. Thefe contractions were capable of being produced, of whatever fubftances the circuit of communication was compofed, provided only they were all conductors of the electric fluid. The effects were found to be much increafed by applying a metallic coating to the nerve. He found this peculiar fpecies of electricity to exift in a great variety of animals, and that the contractions may be excited

Proper *galvanifm*, or contractions without excited electricity.

* Sultzer in his Theorie des Plaifirs, quoted by Fabbroni, Philof. Journal, quarto IV. 120, mentioned the tafte by contact of two metals.—N.

either

Theory of *Galvani*; that the interior of the muscle is charged plus, and the nerves form a communication with the outside.

either in the whole body or in particular parts of it, as long as the animal possesses any remains of vitality. Galvani supposed these phenomena to be analogous to the effects of the Leyden phial; that there is an excess of electric fluid in the interior of the muscle or in the nerve, and a deficiency on the outside, or *v v.* The nerve he conceived to act the part of the wire in the Leyden phial. Soon after the publication of Galvani's work, Valli's Letters on Animal Electricity appeared in the Journal de Physique, vol. 41. & seq.

Valli's Letters on Animal Electricity, 1793.

Volta's Letters to Cavallo, 1793.

In the Transactions of the Royal Society for 1796, Volta's letter to Cavallo was published, which besides giving an account of Galvani's discoveries, contains many original experiments and observations. The analogy of the Leyden phial he shows is without foundation, for he found that he could excite similar contractions in the limbs, when the conducting circuit only touched two parts of the nerve, two muscles, or two parts of the same muscle; in order to accomplish this, it is however necessary to use two different metals. He supposes that in these cases the muscular contractions are produced by a small quantity of electricity which is excited by the action of the metals upon each other; this he conceives to depend upon a general law of the electric fluid, and that its effects are visible in the experiments of Galvani only because the prepared animal is the most delicate species of electrometer. Volta endeavours to prove by experiment, that the action is always in the first instance upon the nerves, and that the muscles are only affected through their medium. He imagines that it is not necessary that a communication should exist between the nerves and the muscles according to the opinion of Galvani, he imagines that the contractions will be produced in the limbs, if the influence be only made to pass from one part of a nerve to another part of the same. But we shall find that in this idea Volta is probably mistaken, as in his experiments the moisture adhering to the nerve formed a communication between it and the muscle. He found that if different parts of a nerve, or indeed if the body of the animal in general, be laid upon two different metals, and these metals be made to communicate by a conducting substance, muscular contractions are produced. These experiments succeed with more certainty when the skin is removed; this precaution is more especially requisite if the animal have a dry skin, as is the case with birds and quadrupeds.

No charge in the animal.

but electricity from the contact of different metals applied to the nerves.

The galv. action supposed to exist in nerves only.

peds. In a second letter which is printed in the same vol. of the Phil. Transf. Volta pursues his experiments and observations upon the subject of galvanism. If a single muscle, or a part of it be armed with coatings of two different metals, and these be made to communicate by a conductor, the contractions will be excited, but no effect will be produced if two coatings of the same metal be employed. Worms and snails he found could not be excited by this influence; but flies, beetles, grasshoppers, and butterflies, he found were subject to its action. Upon the whole it appears that those animals only which have distinct limbs, with flexor and extensor muscles, are excitable by animal electricity. In those animals which are acted upon by the galvanic influence, Volta found that it was only the muscles which are under the direction of the will that can be made to retract; in his experiments he never perceived that the heart was affected by the two metals, though this organ is thrown into strong contractions by the slightest chemical or mechanical stimulus. The two metals which were found by their union to perform the most powerful effects were zinc and silver. Volta placed these metals one on each surface of the end of the tongue; when they were brought into contact no motion was produced, but a strong sensation of taste was excited. When the metals were applied to the root of a tongue cut from the mouth, contractions were produced.

Of the various animals, those only were found to be affected which have distinct limbs, &c. and those muscles only which are subject to the will.

Zinc and silver.

In the same year in which Volta's letters appeared in the Phil. Transactions, Fowler published an essay on Animal Electricity; these works must therefore be considered as equally original. The extracts given above from Volta's letters, prove that he considered the phenomena of galvanism to depend upon the operations of the electric fluid; other experimenters however conceived it impossible to reconcile the new discoveries with their previous ideas of the nature of electricity, and Fowler commences his treatise by this enquiry. In order to ascertain this point he investigates the circumstances which are necessary to the production of the muscular contractions. These he found to be the contact of the two metals with each other, and their communication with the animal; the contractions may also be produced by bringing the metals in contact with each other in water, without either of the metals touching the animal. When the metals are applied to the

Fowler's Essay, 1793, Whether galvanism be electricity.

Conditions of the effect: that two different metals should touch each other and the animal; viz. the nerve and muscle;

either immediately, or mediately by water.

the nerve alone, Fowler ſtill conſiders it requiſite that there be a connection between the nerve and the muſcle; in ordinary caſes this connection is effected by the moiſture which adheres to the nerve, a circumſtance which Volta ſeems to have overlooked. Valli had endeavoured to form a theory of animal electricity founded upon the idea that the electric fluid was unequally diſperſed through the body, and that the application of the metals produced an equilibrium; Fowler performed a number of experiments to diſprove this idea, and apparently with ſucceſs. He concludes the firſt part of his eſſay by giving it as his opinion, that the phenomena of galvaniſm are not reconcileable to the known laws of electricity, becauſe for the excitement of the electric fluid, motion between an electric and a conductor appears to be neceſſary, whereas in theſe experiments two metals only are the ſubſtances employed. He alſo failed in cauſing charcoal to conduct this influence, though it is a better conductor of electricity than the animal fluids; in this particular, however, he appears to have been miſtaken. He conſiders the influence as very ſimilar in its effects to the action of the torpedo, though not altogether the ſame. He was unable to produce the muſcular contractions in worms, yet he found that when a worm or a leech lying upon ſilver, put its mouth upon zinc, it appeared to ſuffer great uneaſineſs. Dr. Fowler in the ſecond place proceeds to enquire, whether all the muſcles of the body be ſubject to this new influence. He found it difficult to excite any contractions in the heart, though at length, by uſing proper precautions, he ſucceeded; but he was not able to excite contractions in the ſtomach or inteſtines. He diſcovered that when the nerves of viſion were acted upon by the two metals, a ſenſation of a flaſh of light was perceived in the eye. When inflammation was excited in a limb, it appeared to acquire additional ſenſibility to the galvanic influence. Fowler's work concludes with a letter from Profeſſor Robiſon, who made ſome new obſervations upon the production of the galvanic flaſh, and firſt noticed the effect produced by applying the tongue to a number of pieces of ſilver and zinc alternately piled upon each other.

Attempt to diſprove the theory of an electric charge in the animal ſyſtem; and to ſhew that galvaniſm is not electricity.

Torpedo.

Effect on worms, and on the involuntary muſcles.

Diſcovery of the flaſh of light.

Profeſſor Robiſon makes a pile of zinc and ſilver.

Darwin in 1794 conſiders galvaniſm as electricity.

In 1794 the firſt volume of the Zoonomia was publiſhed; Darwin ſpeaks of the phenomena of galvaniſm, and conſiders them as electrical. The muſcular contractions he ſuppoſes depend

depend upon the fenfibility of our nerves to fmall quantities of the electric fluid. Bennet difcovered by means of his electrometer, that zinc, when feparate from other metals, always is in the minus ftate, and filver in the plus ftate; when therefore they are brought nearly together, a fmall plate of air is charged like a Leyden phial; when the metals are brought into contact this is difcharged.

Bennet's early difcovery of the plus ftate of zinc (1789.)

In the Phil. Tranf. for the year 1795, is a Paper by Dr. Wells on the fubject of galvanifm. He propofes for confideration the three following enquiries; Do the contractions obferved by Galvani depend upon any property peculiar to the living body? What are the conditions neceffary for the excitement of the influence? Is it electrical? With refpect to the firft queftion, he fuppofes that animals act only on account of their moifture. With refpect to the fecond, he found that one metal and charcoal excited the contractions as readily as two metals; in this he corrects the miftake of Fowler noticed above. He however found that all charcoal will not act as a conductor of the influence, in which opinion Volta agrees with him. Wells does not agree in the hypothefis adopted by Volta and others, that the contact of the metals produces an alteration in the difpofition of the electric fluid, for he very properly afks, why fhould not the natural moifture of the animal afford a communication between the two metals before they are connected by any other conductor? He farther difcovered, that contractions could be excited by one metal only when it had been rubbed upon another metal, or even upon the hand. Charcoal may by the fame means be made to produce the fame effect; he proved by a variety of experiments that the friction does not in thefe cafes communicate electricity to the metal or the charcoal; we may conjecture that an incipient oxidation, or fome other chemical change was produced upon the furface of thefe fubftances. With refpect to the third queftion, Wells is decidedly of opinion, that the phenomena are electrical; the influence being conducted by all conductors of electricity, and by them alone.

Dr. Wells, 1795, ftated from experiments that animals are affected only by means of humidity; that the ftate of the metals is not altered by contact; but that the phenomena are electrical.

Befides the works here mentioned a number of communications appeared in the different fcientific journals of Italy, France, and Germany, and feveral diftinct treatifes were publifhed on the fubject of animal electricity. Humbolt particularly

Various other papers, Humboldt, Dr. Monro, the Inftitute of France, &c.

ticularly diſtinguiſhed himſelf by his aſſiduity in varying the experiments. Dr. Monro wrote upon the ſubject, and the National Inſtitute of France publiſhed an elaborate report, drawn up by a Committee compoſed of ſeveral of its moſt learned members. As however theſe works do not appear to contain any facts which materially illuſtrate the nature of the galvanic influence, or lead us to form any more accurate notions reſpecting its operations, we ſhall in this brief ſketch only notice the papers of Fabroni, of which an account may be found in the 4th Vol. of Nicholſon's Journal. He deduces from his experiments that galvaniſm is intirely a chemical phenomenon; he finds that metals become oxidated when in contact with each other in circumſtances where this would not take place if they were kept ſeparate. This idea he confirms by many obſervations and experiments, and ſuppoſes that when the galvanic influence is excited by the action of two metals that a chemical affinity is exerted; he does not however point out very clearly in what manner the chemical action which is exerted by the metals can be connected with the phenomena of galvaniſm.

Fabroni: that galvaniſm is merely a chemical phenomenon.

In the year 1789, Cavallo publiſhed a new edition of his Electricity, and he added a good account of the principal facts in galvaniſm; an ample hiſtory of this ſcience may be alſo found in the Supplement to the Encyclopædia Britannica.

Hiſtory of this power in Cavallo's Electricity, and the Supplement to the Encyclopædia Britannica.

In this ſtate our knowledge of galvaniſm continued until the beginning of the year 1800, when Volta made his diſcovery of the apparatus uſed called the Galvanic Pile. In the Phil. Tranſ. for that year is a letter from Volta containing an account of the pile, and a detail of many curious experiments which he had performed with it. This inſtrument conſiſts of a number of circular pieces of two different metals, laid alternately upon each other, with a piece of moiſtened paſteboard or ſkin interpoſed between each pair. The metals which anſwer the beſt for this purpoſe are zinc and ſilver, which were found to be the moſt powerful in exciting the muſcular contractions in the former experiments. If the two pieces of metal which form the extremities of the pile be graſped firmly in the hands previouſly moiſtened, a ſhock will be felt through the hands and arms, more or leſs powerful in proportion to the ſize of the pile. This ſhock may be repeated as long as the

Grand diſcovery of the pile by Volta.

Deſcription.

Galvanic ſhock.

the pasteboard between the metals retains its moisture. Volta conceives that this apparatus in every respect resembles the electric organ of the torpedo and gymnotus electricus.

As soon as this discovery became known in England, a variety of experiments were performed with the new apparatus, and many very interesting and important facts were discovered: these are for the most part detailed in Nicholson's Journal, Vol. IV. & seq.

Carlisle and Nicholson determine the electricity of the pile, and discover its *chemical action, and the decomposition of water,* into hidrogen and oxigen.

In the 4th vol. p. 174, is a paper written by Mr. Nicholson himself. He begins with a description of Volta's pile, and then relates the results of some experiments which were performed upon a similar one by himself and Mr. Carlisle. By using the revolving doubler they found that the electricity was minus in the silver end of the pile, and plus in the zinc end. Mr. Nicholson proposed that the influence should be permitted to pass from one end of the pile to the other through a tube of water; for this purpose a division was made in the conducting wire, which was composed of copper, and the two ends of it were terminated in a small tube of water. Immediately the wire connected with the silver end of the apparatus began to produce a gas, which was found to be hidrogen, while at the same time the wire connected with the zinc end became oxidated. Recourse was then had to a wire of platina which is not oxidable, and immediately gas began to be evolved from both ends. Upon examining the gases separately, that from the silver end was hidrogen, that from the zinc end oxigen, and they were generated nearly in the proportion requisite to produce water.

Importance of this experiment.

This experiment, as well with respect to its immediate effects, as with regard to the consequences which may be deduced from it, may, I think, be justly considered as the most important that has occurred since the discovery of oxigen by Dr. Priestley.

Electric spark visible.

The electric spark was distinctly visible in these experiments.

Cruickshank. Effect on chemical tests.

Mr. Cruickshank relates his experiments, in which he made use of the interrupted circuit after the manner of Mr. Nicholson; he caused the influence to pass first through distilled water tinged with litmus, and afterwards through water tinged with Brazil wood. In the first case there was a redness produced by the zinc, and in the second by the silver wire. By submitting a portion of water for a long time to the action of the pile, a sensible diminution was observed in its bulk. He observed the

the production of hidrogen and the oxidation of the zinc end when he used a communication of copper; and when he employed a wire which was not oxidable, he obtained both hidroden and oxigen according to the observation of Mr. Nicholson.

Reduction of metals from their solution.

He permitted the influence to pass through a solution of a metallic salt, and found after some time, that the metal began to be revived at the silver end of the apparatus.

Mr. W. Henry decomposes sulphuric and nitric acids, but not the muriatic.

Mr. William Henry, of Manchester, submitted concentrated sulphuric acid to the influence of galvanism: he made use of a wire of platina. Not only the water which always enters into the composition of this acid, but also part of the acid itself, appeared to be decomposed, as the oxigen which he procured was in a larger proportion to the hidrogen obtained than is sufficient to compose water. Nitric acid was also decomposed, and the water of muriatic acid.

Oximuriatic was deoxigenated.

When oximuriatic acid was subjected to its influence, the water was decomposed, and the acid was deoxigenated.

Gases could not be subjected to it.

Gasses do not appear to be conductors of this influence, so that Mr. Henry was not able to try its effects in decomposing muriatic acid gas.

Ammonia was decomposed.

It also appears to have the power of decomposing ammonia.

(To be continued.)

*** On account of the great Quantity of original Communications, the *Scientific News and Account of Books* is necessarily deferred.

X. Y. Z. *is informed that the derivation of chrome or chromium is from* χρωμα, color; *and was adopted by Vauquelin, because its compounds with oxigen are coloured. See Philos. Journal, quarto, II.* 444.

Mr. Boswell's improvements in the hydraulic Engines,

Fig. 3. Presſure Engine.

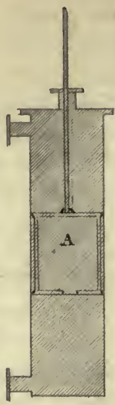


Fig. 2. Mr. Goodwyns.

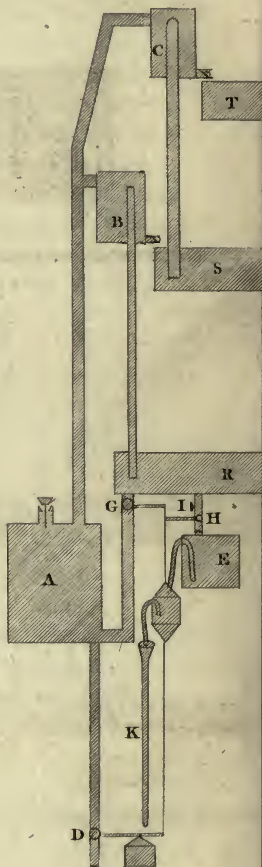
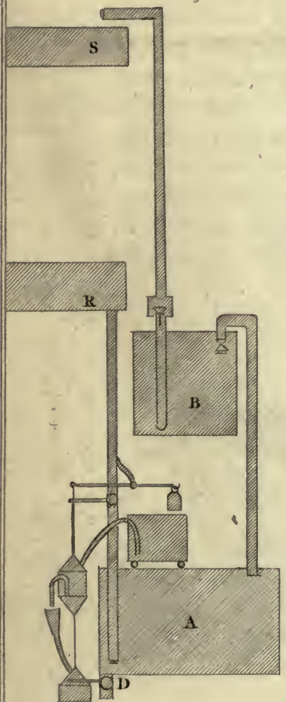
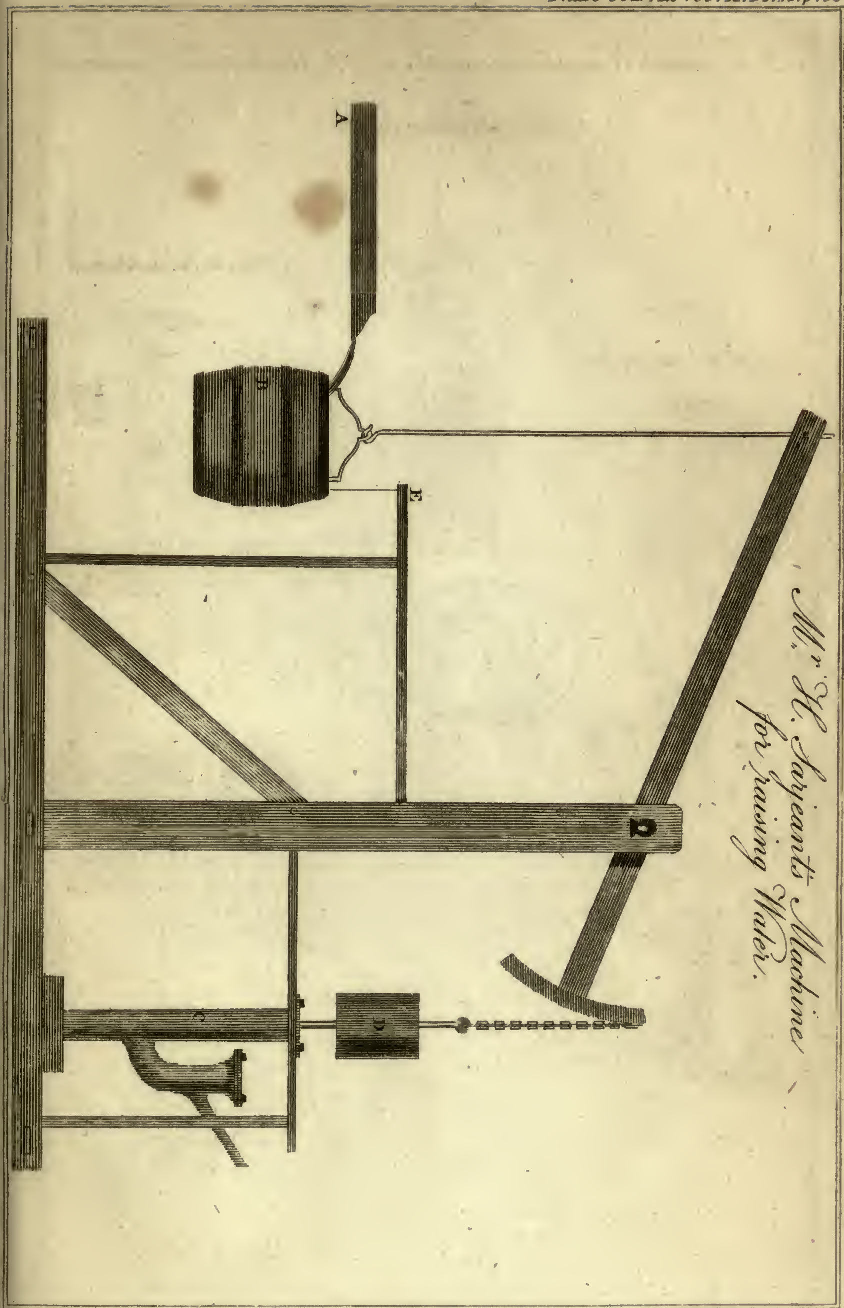


Fig. 1. at Schemnitz.





Mr. H. Sargeant's Machine for raising Water.

Mutlow Sc. Russell Ct.

Stove of Cit. Guyton.

Fig. 1.

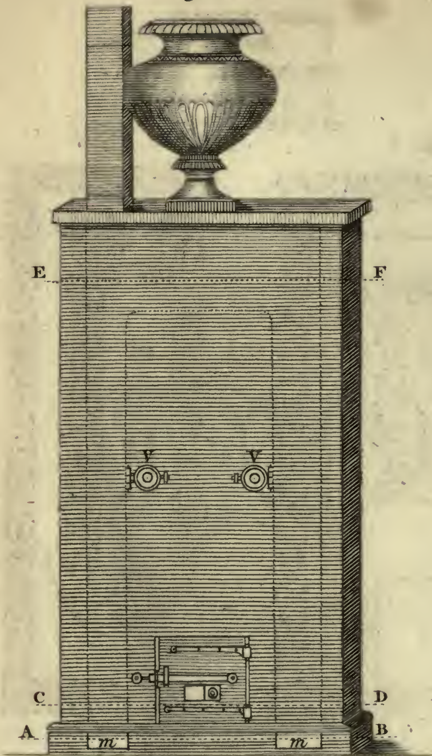
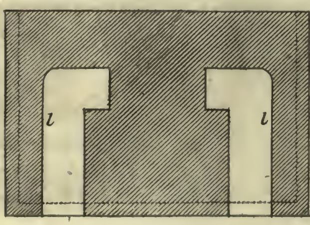


Fig. 2.



Section of the Stove of Guyton.

Fig. 7.

Fig. 4.

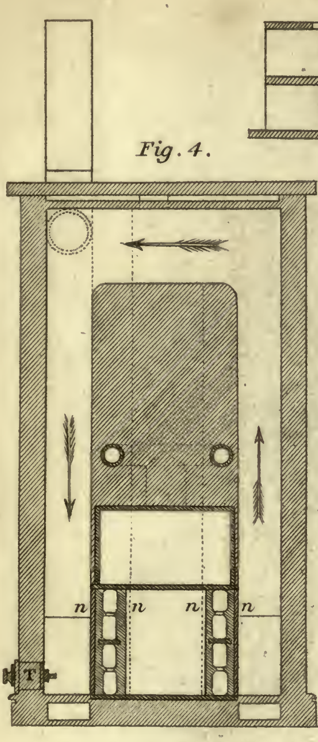


Fig. 6.

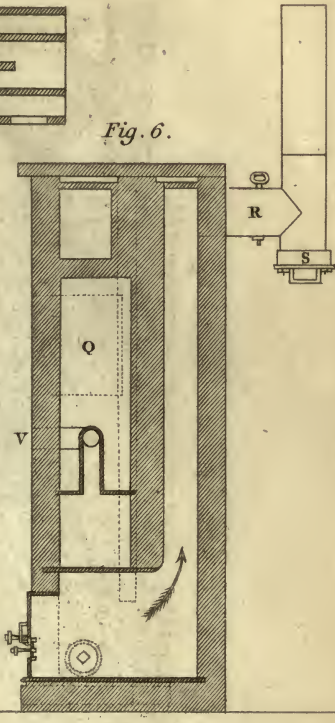


Fig. 3.

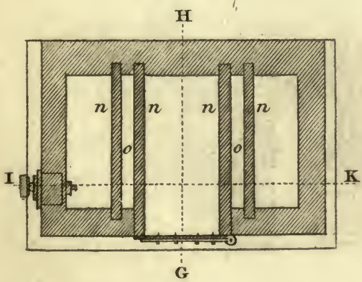
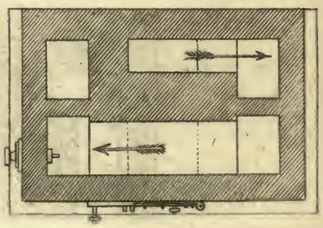


Fig. 5.



Mutlow Sc. Russell Ct.

Strong framed Levers for steam Engines by Mr. J. C. Hornblower & others.

Iron Cradle.

Fig. 1.

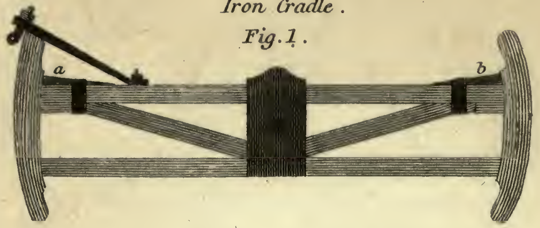
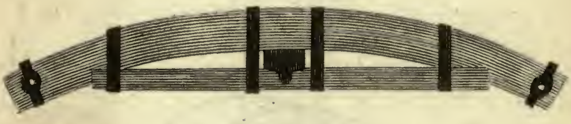


Fig. 2.



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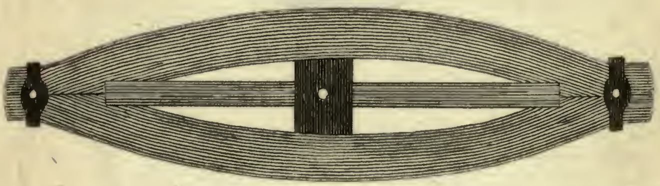
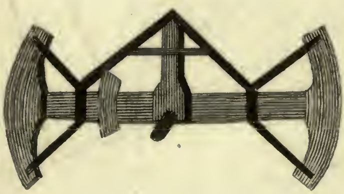


Fig. 4.



Mutlow Sc. Rufsell Co.t

Dr. Young on the theory of Light & colours.

Fig. 2.

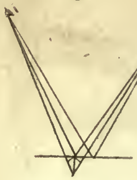


Fig. 3.

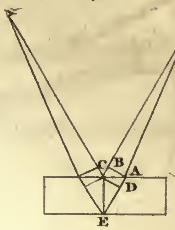


Fig. 1.

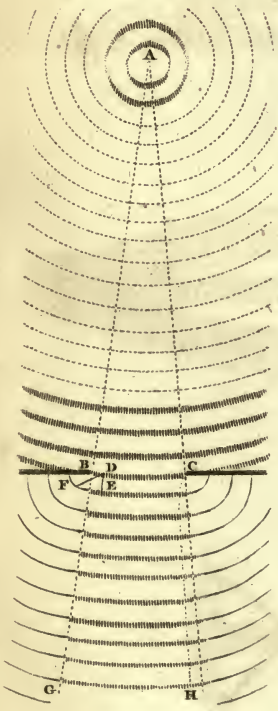
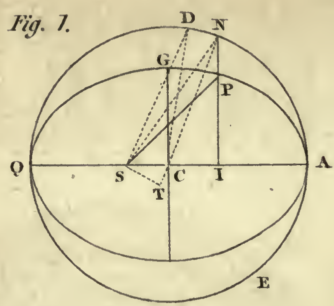


Fig. 4.

Mutlow Sc. Russell Ct.

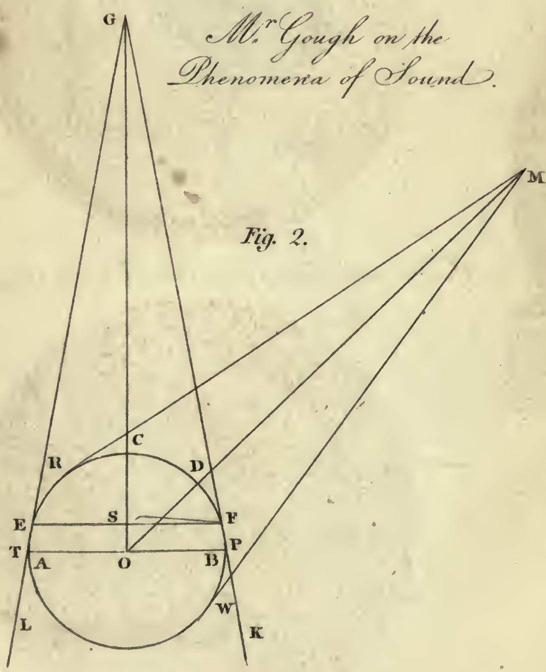
Mr Gregory's Investigation of Mr Pearson's Analogy, &c.

Fig. 1.

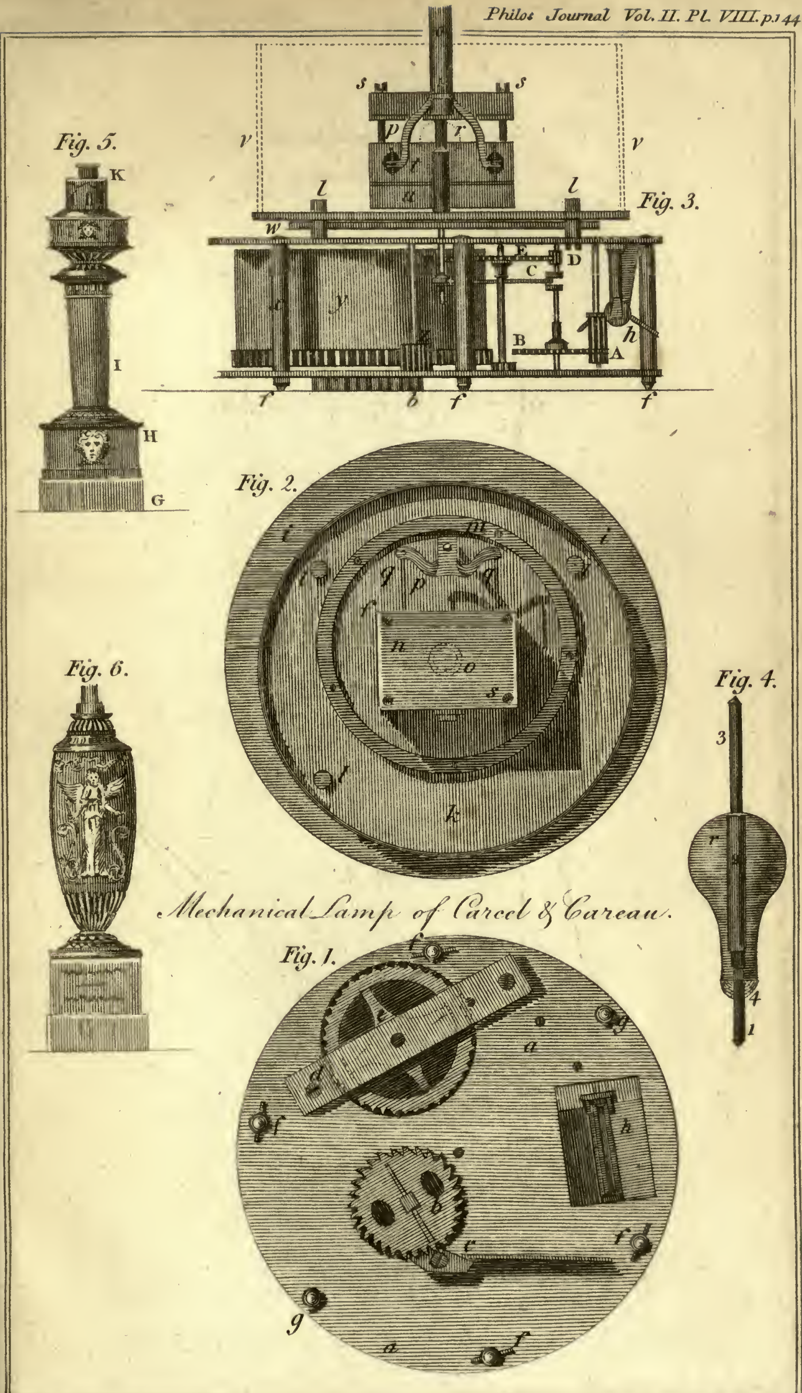


Mr Gough on the Phenomena of Sound.

Fig. 2.

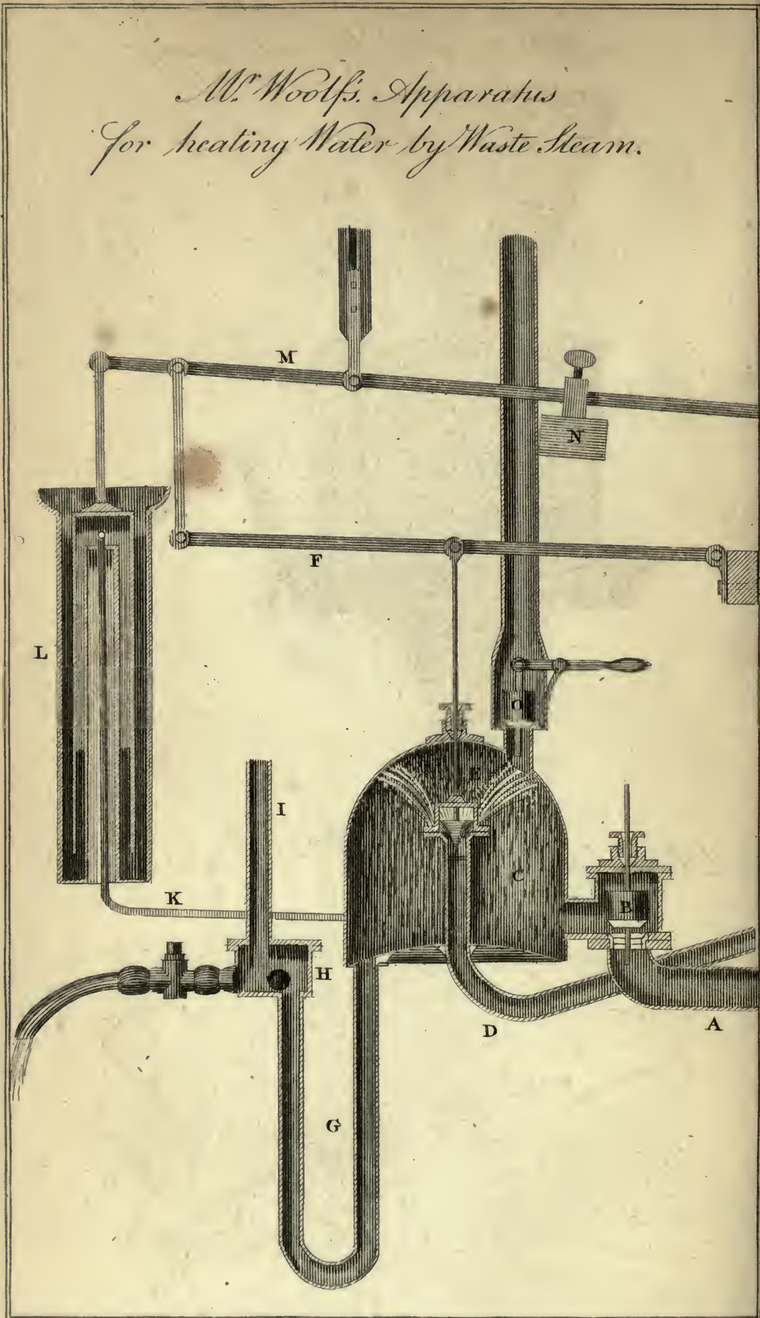


Mulow Sc. Russell C°



Mechanical Lamp of Carcel & Careau.

Mulow Sc. Russell C°



Ma..low. Sc. Russell Ct.

Mill by Mr. Garnet Terry.

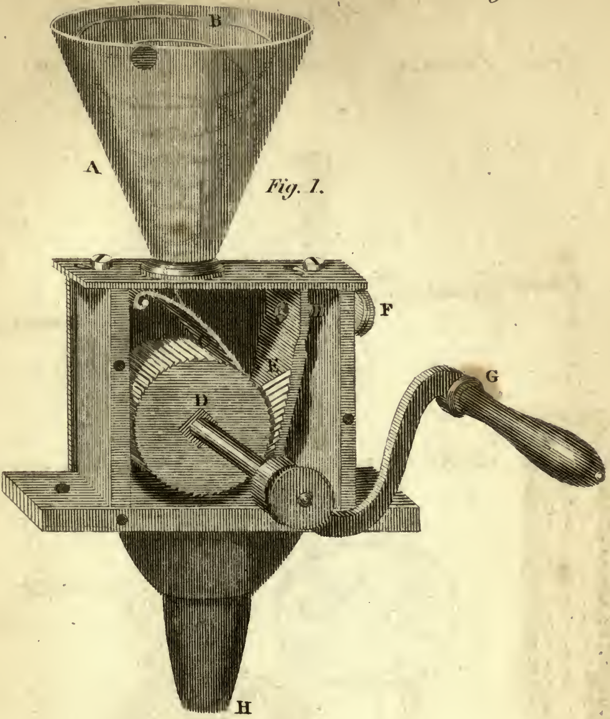


Fig. 1.

Drawback Lock by Mr. Bullock.

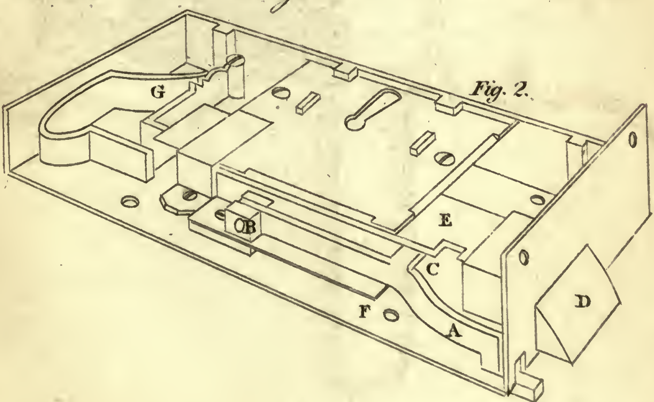
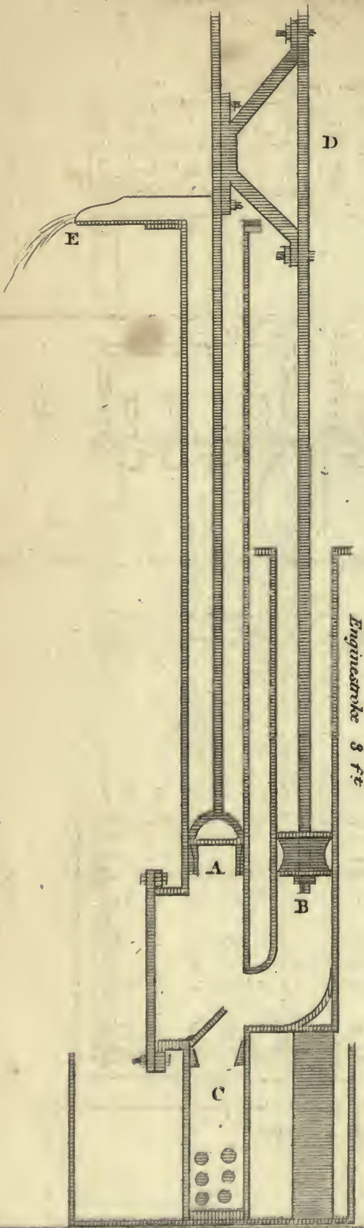


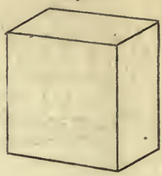
Fig. 2.

Method of applying a temporary forcer, by Mr. Trevithick.



Crystals

Fig. 1.



described by Cte. de Bournon.

Fig. 2.

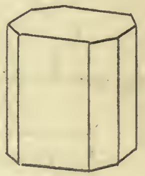
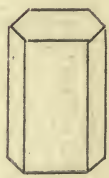


Fig. 3.



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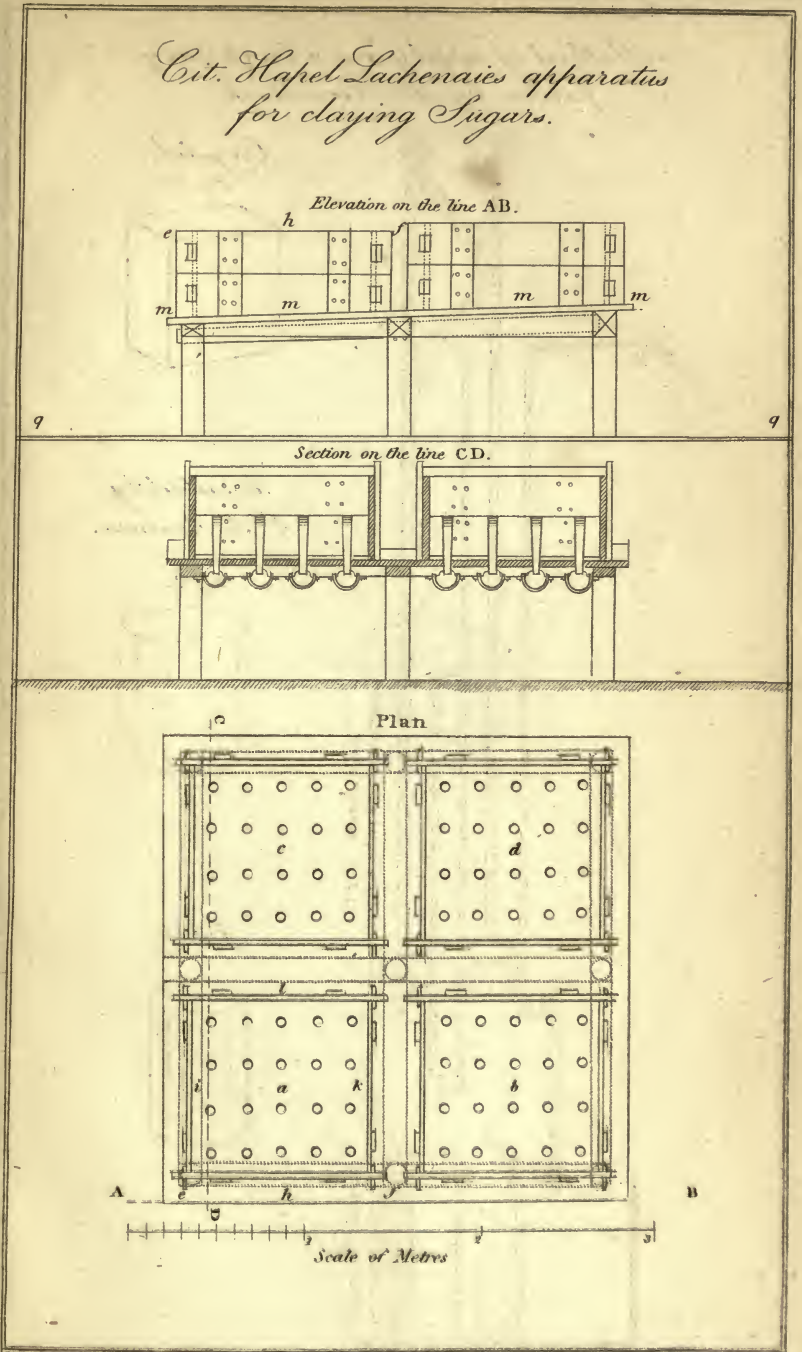
Cit. Hapel Lachenaies apparatus
for claying Sugars.
Elevation on the line AB.
e
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Section on the line CD.
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1
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Scale of Metres

Mr. Robt. Jameson's Observations on the Formation of Granite.

Fig. 1.

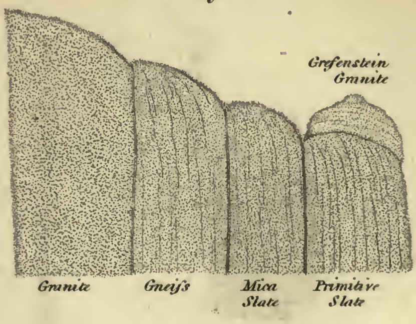
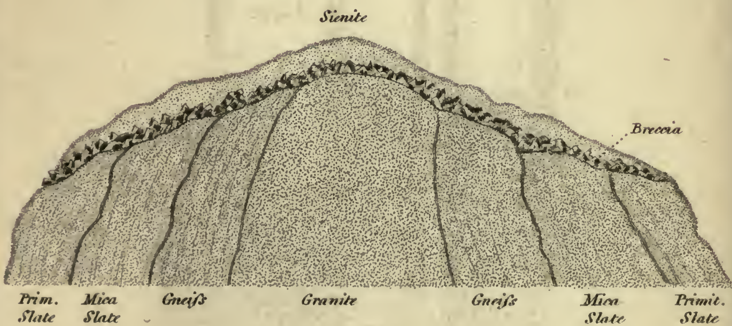


Fig. 2.



Hallow Sc. Russell Ct.

Mr. Banks's Experiments on the velocity of effluent Air or Gas.

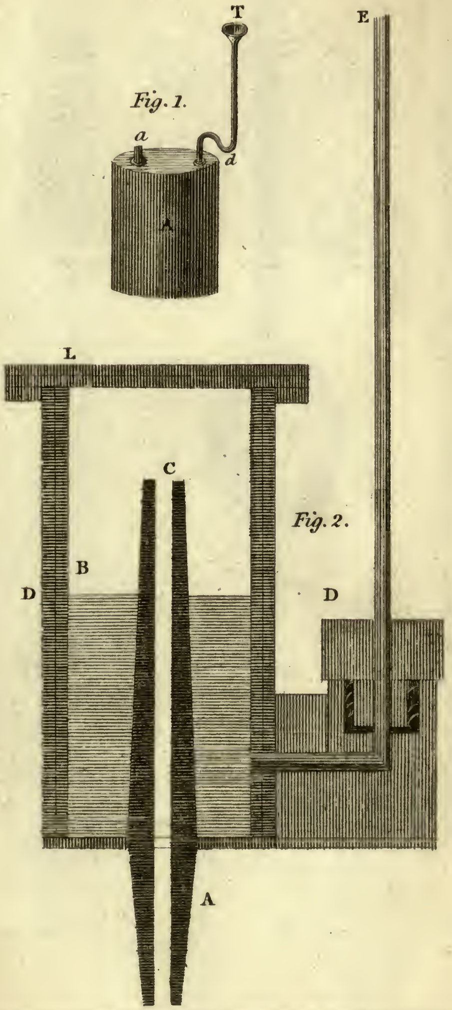


Fig. 1.

Fig. 2.

Atkins's Spirit Hydrometer.

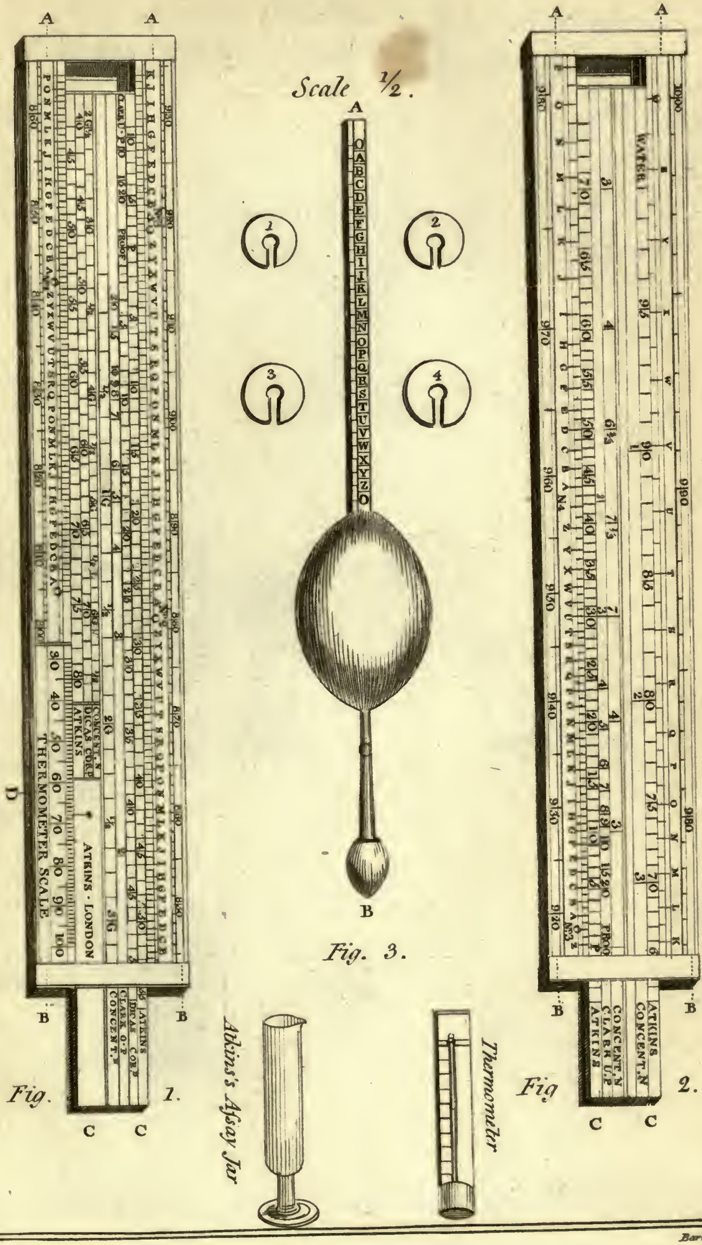


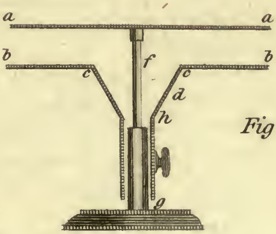
Fig. 1.

Fig. 3.

Fig. 2.

Barlow sculp.

Compound Condensers of Electricity:
By Mr. Reed and Mr. Cuthbertson.



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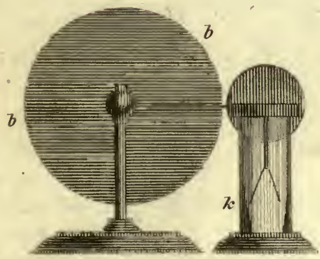


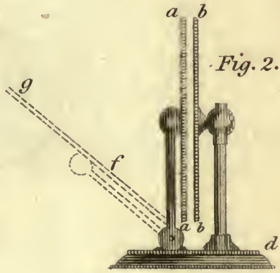
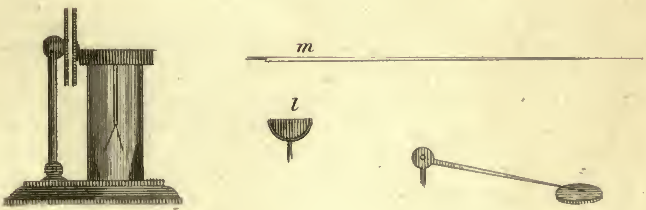


Fig. 2.

Fig. 3.



Mutlow Sc Russell Cot.

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D.

E.

Read,

END OF THE SECOND VOLUME.

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